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THE REVOLT OF THE BIOCHEMISTS[1]

By Dr. P. A. LEVENE

THE ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH, NEW YORK

MAY I begin my remarks of this evening by acknowledging my gratitude to the men to whom I owe the great honor of having my name added to the truly illustrious names of the Willard Gibbs medalists who have preceded me. I am referring to the Board of Scientific Directors of the Rockefeller Institute for having liberally supported the work of the Chemical Division and to Dr. Simon Flexner for his help and encouragement, and then to those who have participated in the work of the Biochemical Division of the Rockefeller Institute, some for a longer and some for a shorter period of the twenty-seven years of the existence of the chemical laboratories.

To your section of the American Chemical Society and to the committee of award, I owe a special debt of gratitude, for I accept the medal not as a personal tribute but as an expression of recognition of that branch of science to the progress of which we have devoted our energies.

To-day, this branch of science is in need of encouragement. Even in European countries where biochemistry has had a long and glorious record and a great tradition, it is held somewhat in disfavor today. The story of the rise and fall of biochemistry in the esteem of the higher scientific hierarchies is in a way connected with the incident of the revolt of biochemistry against the concept of vital force or, as the Germans call it, "Lebenskraft." This was a revolt against restriction of the exploits of the human mind, for, modest as the domain of biochemistry may be, it had to align itself with some more universal philosophy in order that it might remain in the family of sciences.

Until nearly the middle of last century, every chemist was a biochemist. Chemical hierarchies did not yet exist. All natural substances whether of mineral,

[1] Address on the occasion of the acceptance of the Willard Gibbs medal.

vegetable or animal origin were included in the scope of interest of every chemist. Chemistry was a purely descriptive science. Substances were the subjects not only of curiosity but also of affection. Theory, whatever there was of it, played a subordinate part in the life of the chemical investigator. His mental attitude was that of a collector of "rarities." Indeed, how else could one understand the reactions of a man of the type of Scheele? In a critical period of his life a position was offered to him as head of an apothecary shop in a small town, Koping, in Sweden. An inferior competitor snatched the job from him; whereupon his friends in his native land, as well as in other countries, set out to secure a more advantageous post for Scheele. Offers came from Berlin, from England and from Upsala. And what was Scheele's reply?—"I can not eat more than my appetite permits and if I can find enough bread in Koping, is there any need for me to search for it elsewhere?" The life of Scheele was truly serene. He owed comparatively little to the theoretical legacy of the past and had no obligation to the generations to come.

Scheele, himself, however, made the task of the generation which followed him more difficult, for he did something which contained the seeds of a new era. In the year 1777, Bergmann wrote that organic synthesis was beyond any hope of success, but in the year 1782–1783 Scheele prepared potassium cyanide by fusion of graphite, potash and salammoniac (NH_3HCl) and thus truly was the first to prepare synthetically a substance containing the element carbon linked to nitrogen and thereby to accomplish the first organic synthesis. But the significance of the achievement was not recognized at the time. Indeed, Fourcroy, in his famous text-book which appeared over 20 years after Scheele's death, maintained that whereas minerals are the products of ordinary physical forces, organic substances owe their origin to a force of an entirely different category, namely, to an organic vital force. Whereas the first were common natural forces, the latter were characterized by purposiveness. This definition does not sound much different from the one advanced by the modern Neovitalists.

Not Fourcroy alone but all great chemists of the early part of the last century were orthodox believers in the doctrine of vital force. Thus Berzelius believed that the secret of the origin of organic compounds lay not in the elements which entered into their composition, not in the element carbon, but in the fact that they were produced by an organism, or rather by organs of a living organism, and the term "organic" was meant to emphasize their origin. In the year 1827, Berzelius was still of the belief that the organic compounds could never be synthesized in the laboratory. Yet only a year later his own student, Wöhler, accomplished the synthesis of urea. "Did this reaction mean a transformation of inorganic material into an organic substance?" was the question which he asked of his old master, Berzelius. The answer was polite but non-committal.

To us to-day, the year 1828 stands for the date of the great revolt of the biochemists against the "Lebenskraft," or the "Spiritus Vitae." In reality, the true revolt came much later. Tradition is too comfortable an armor to be cast off at the first assault. Berzelius continued to refer to the synthetic substances as "incomplete imitations of the organic products." Gerhardt, one of the most brilliant theoretical chemists of the middle of last century, in 1842 maintained that the vital and the chemical forces are of antagonistic nature, the former accomplishing the synthetic functions and the latter that of degradation. Under normal conditions of life, the two forces are balanced; after death, the chemical forces of disintegration are unchecked.

Finally, doubts crept into the minds of some of the chemists. Thus, we find Liebig was ready to make concessions to the new tendencies. He wrote, "Under the influence of a non-chemical agent (Life, Vital Force) chemical forces also function in the organism. Through the guidance of this dominant force and not independently, elements arrange themselves into chemical substances such as urea in the manner in which the intelligent will of the chemists forces them to unite outside the organism. Hence, it will be possible to prepare in the laboratory quinine, caffeine, plant pigments and dyes but not a cell, muscle fiber, or a nerve fiber." Mulder, another biochemist of lesser magnitude as a contributor, but an able thinker, writing on "Organic Forces" states that the assumption of the existence of a peculiar vital force (Lebenskraft) was not supported by experience and furthermore he defends the thesis that the organic forces of the complex substances pre-existed potentially in the elements composing them.

All these utterances, however, were peaceful philosophical speculations lacking the momentum necessary to produce a real revolt against the authority of the vital force. Finally, the revolt broke out effectively in 1860, when Berthelot wrote, "The objective of our science is to banish 'Life' from the theories of organic chemistry." The successes in organic synthesis were behind the authority and the power of this utterance.

The end of last century continued vibrating with the enthusiasm contained in the words of Berthelot. The hopes and expectations of chemists then knew no bounds. Not organic substances alone but organized living matter seemed within reach of the synthetic

method. The chemist was not isolated in his hopes and expectations. Physiologist, general biologist, physicist, psychologist and metaphysician were all under the same spell of mechanistic philosophy.

The strides made by organic synthesis for a time surpassed all expectations. It suffices to read the public utterances of Emil Fischer, perhaps the most daring chemical virtuoso and at the same time the most cautious man with respect to theory or prediction, in order to realize the depth of his conviction that organic synthesis would penetrate into and would reveal the mysteries of living matter, if not of Life itself. Indeed, in one of our conversations, he expressed belief in the possibility of synthesizing enzymes.

But as our century advanced, a change came in the general attitude of men of science. Mechanistic philosophy fell into disrepute and with it biochemistry, as one of the foundation stones of mechanistic biology, lost its prestige. Vitalism again came to the front, under the name "Neovitalism." The truth is that the "Neo" philosophies divorce the inorganic forces from the organic as much as the old vitalistic philosophy had done. The monism of the mechanistic philosophy is banished and the "Lebenskraft" is reinstated.

Shall this state of mind of the philosophies of our day alter the attitude of biochemistry? Shall chance, probability, indeterminism become the foundation of the philosophy of biology as they are of the philosophy of the physical world? Shall life forever remain a word without an accurate definition? The retreat is cut off for the biochemists as it was for the revolting angels of Anatole France.

True, for the individual worker, there is some justification for having moments of depression. The achievements of biochemistry may seem disproportionate to the effort, when one thinks of the time it took to unravel the structure of one substance alone. Uric acid was one of the first biological substances to have interested seriously the biochemist. It was discovered independently by Bergmann and by Scheele in 1776; yet only in 1898 was the knowledge of its architecture fully attained by Emil Fischer. Hemoglobin has a similar history. It was in the year 1849 that a biologist saw, under the microscope, its beautiful crystals, but not until 1929 was the architecture of hemin unraveled by Hans Fischer, while the entire structure of the crystals seen by Leyden in 1849 is not yet known. Still more discouraging is the history of proteins. These substances were known since the earliest times. In 1860 the term "protein" was introduced by Mulder, who already at that time speculated on their structure. Yet how little do we know about the details of the structure of a single protein; and the number of them in nature is endless. Think of the chromatins, which are supposed to be the carriers of heredity and of reproduction! They were discovered in 1869 by a very able biochemist, Miescher, who, in his turn, was inspired by a biologist, His, and the structure of the substances is not yet known in every detail. Again, it is discouraging to think of the inadequacy of our information about the structure of starch, cellulose, gums and similar substances. Even in regard to glucose, a substance of very simple composition, and a most common component of our daily diet, the knowledge of the details of its structure is not yet complete, though the substance was obtained in pure crystalline form in 1660 by Glauber.

The science as a whole, however, taking stock for the period of one hundred years, will find a record of achievement which once seemed unattainable. It is enough to compare the humble attitude of Liebig and Wöhler with the daring of Fischer. Having made the discovery that urea constituted a part of the molecule of uric acid, Liebig and Wöhler concluded that uric acid is a complex of urea with a *second radicle* the nature of which in all probability would never be revealed. And yet they had in their hands enough data to permit the formulation of the molecular architecture of the substance, had chemical theory been advanced to its present state. And indeed, Fischer, having accomplished the task which Liebig and Wöhler thought unattainable, attributed his success not to any special individual merit but to advances in theory and in the technique of organic chemistry of his day. To-day, it may be taken for granted that the discovery of the structure of every natural organic substance is only a matter of time and organization. The tools for this aim are in our hands. New achievements in this direction no longer will contain the element of surprise and it is not expected that they will reveal deeper secrets of the mystery of Life than those already in our possession.

Had biochemistry discovered no new ways, no new methods for attaining the aim formulated by Berthelot, namely, to banish Vital Force from chemical theory, then the "Neovitalist" might have been justified in his pessimism in spite of the past achievements of synthetic biochemistry. However, biochemistry to-day has opened new avenues of approach towards its goal. If the interest of the biochemist of the past was structure, that is, the static state of the molecule, the interest of the biochemist of to-day is the functional side of the molecule. It may be remarked that this approach is not entirely new; but in the past it stood in the background, whereas to-day it is the dominant concern of our branch of science. True, as soon as inorganic catalysts were discovered, Berzelius advanced the idea that life phenomena are

the resultant of the play of catalysts, though of a different category from that of the mineral catalysts. In the present-day terminology they would be referred to as "biocatalysts." Berzelius believed that every living organism, plant or animal, contained an infinite number of these catalysts.

In the days of Berzelius, of Wöhler and of Liebig, the nature of the biocatalysts was as great a mystery as that of Life itself and the quest into their chemical nature held out so very little promise of success that it was tacitly forbidden. To-day, the chemical nature of at least some enzymes is no longer a matter of mystery, and much unexpected information has been obtained regarding the mode of their action. It is known to-day that the action previously attributed to a single agent, a single biocatalyst, is in reality the product of the combined action of a group of agents. Biocatalysts, or enzymes, as they are often called, are characterized by their instability, particularly with regard to heat, and these very unstable components, playing an important part in all life phenomena, are often powerless to act in the absence of definite accessory agents which are known under different names, as coenzymes, complements, activators or kinases. The chemical nature of these last substances is one of the foremost problems of to-day. Many of them have been isolated and have been found to be fragments of the more complex substances, being, for example, simple peptides which are fragments of proteins, or simple nucleotides which are fragments of nucleic acids, while others have been found to be of still simpler composition. These simple substances do not play the part of accessories only, but in some instances exercise a directive influence, causing one and the same enzyme to function in one of several possible directions. It may suffice in this place to refer to the lipases, the fat-splitting enzymes, which were made to produce dextrorotatory or levorotatory acids depending upon the choice of inert substances added to the reaction mixture. These discoveries are very significant inasmuch as eventually they may lead to a revision of our views on the great multiplicity of the biocatalysts.

The mechanism of the action of the accessory substances is not the same in every instance. They may activate either the enzyme or the substrate or both in order to bring about a coupling between substrate and enzyme. In the cases of fermentation and animal combustion of sugar such an alteration of the substrate (sugar) is generally accepted and certain information regarding the structure of the altered fermentable sugar already is available. Thus, again, new problems are created, those of the chemical differences between the stable molecules which were the concern of the older biochemists and the unstable molecules which are the concern of the newer biochemists. The change in stability of certain substances can be produced now by chemical means. The grape sugar molecule seems to be activated by introducing certain groups such as phosphoric acid in a definite position of the molecule. The stability of the smaller fragments of the protein molecule can be lowered by attaching certain acidic groups to the peptide nitrogen and one exceptionally stable component of nucleic acids is made very labile by hydrogenating one of its parts. By selecting suitable media, the chemist has learned to direct a reaction in a desired sense when two substances may interreact in several different ways. And so one of the most characteristic peculiarities of living matter, the directive force, has been imitated in the laboratory. It is not important that as yet this phase of our knowledge is limited. The significant thing is the formulation of the problem. Its solution is a matter of routine, a matter of the ordinary ingenuity of the human mind.

Granting that the problem of the directive force will be solved, it may also be granted that the entire mystery of life will not be solved by this achievement. Chemistry, however, is already preparing a new attack. A more essential characteristic of living matter than the directive force of individual chemical reactions is the power to coordinate all chemical reactions in such a way that the organism may function as a whole for the purpose of maintaining its normal equilibrium and for the purpose of growth and reproduction. This may be regarded as the integrating force of the living organism. The discoveries of the last decade alone furnish proof of the simplicity of the agents acting towards this end. Think of all the hormones and vitamins! Only those which as yet have not been isolated may be thought of as complex and mysterious. Those obtained in pure state are most generally found to be much simpler in chemical structure than many of the ordinary tissue components and definitely simpler than certain common drugs. In fact, many of them are nothing more than degradation products of common tissue constituents. Thus, it seems that in the living organism the very wear and tear of the living matter makes for its restitution and for its preservation. A decade is only an infinitesimal interval in the life of mankind and without hesitation or doubt, one may predict that the nature of all hormones and vitamins and other biologically important integrating substances will eventually be discovered.

Thus, step by step, one mystery of life after another is being revealed. Whether the human mind will ever attain complete and absolute knowledge of and complete mastery of life is not essential. It is

certain, however, that the revolt of the biochemist against the idea of a restriction to human curiosity will continue. Biochemistry will continue to function as if all knowledge, even that of life, were accessible to human understanding. The past has taught that the solution of one problem always opens up a new one. New discoveries in physics, in mathematics, in theoretical chemistry furnish new tools to biochemistry, new tools for the solution of old problems and for the creation of new ones. So long as Life continues, the human mind will create mysteries and biochemistry will play a part in their solution.

CLINICAL INVESTIGATION[1]

By Dr. FRANCIS G. BLAKE
YALE UNIVERSITY

THE second article of the constitution of this society begins with the sentence, "The objects of this society shall be the cultivation of clinical research by the methods of the natural sciences; the unification of science and the practice of medicine; the encouragement of scientific investigation by the practitioner, and the diffusion of a scientific spirit among its members."

One need not stop to ask, perhaps, to what extent these objects have been advanced in a material sense since the inception of this society twenty-three years ago. The expansion of the clinics during this period particularly with respect to the provision of more adequate hospital wards, laboratories and equipment for clinical investigation has been phenomenal and is familiar to you all. The funds for carrying on clinical research in these laboratories, though undoubtedly not keeping pace with those that have been provided for bricks and mortar, have nevertheless increased surprisingly and perhaps as fast as is wholesome in a period of rapid expansion, when the finding of men suited for research, by reason of a primary interest in the search for new knowledge and the simultaneous possession of those rare, but essential qualities of initiative and originality, is usually more difficult than the finding of material resources. In spite of this difficulty, the number of those engaged in clinical research has likewise multiplied many times during this period, to such an extent, indeed, that the published products of their labors have resulted in a deluge which at times bids fair to engulf us, whether by volume or by depth, I will leave to you to decide.

While these matters need not detain us, it is, perhaps, pertinent to inquire to what extent the character and direction of clinical research and the nature of the methods it employs have been advanced, or perhaps it would be better to say, have been changed during this period of expansion in material facilities and in human activity; whether in fact these more important aspects of clinical research have kept pace with the evident material progress. To do so, it is obviously necessary to have a clear conception of what clinical research is or purports to be, and also just what is meant by the phrase quoted from the constitution—"by the methods of the natural sciences."

Clinical investigation, if "the unification of science and the practice of medicine" be the worthy goal that the writers of our constitution conceived it to be, should not concern itself primarily with physiology or chemistry, with physics, mathematics, or biology, nor even with the application of these subjects by the physiologist, or chemist, or physicist or biologist to the problems of clinical medicine, but primarily with the study of the phenomena of disease by clinicians thoroughly familiar with disease in all its varied aspects through intimate and constant contact with disease in the field—whether this be in the home, the office, the out-patient clinic, or the wards of the hospital should matter little, provided the contact be comprehensive enough to give a reasonably complete picture of the disease in question.

Furthermore, I believe it should be kept in mind that the purpose of this study of disease should be primarily to find out about disease, largely for the fun of doing it, to discover the circumstances or conditions under which disease develops, the nature and mechanism of the disturbances of function and structure which take place during the course of disease, and the circumstances or conditions under which recovery or death occur. Secondarily, this may lead, and fortunately sometimes will, to the discovery of methods of prevention, amelioration or cure, but these practical and humane purposes should, I believe, be kept in the background, if clinical investigation is not to be too soon diverted and frequently misled in following its main purpose, the elucidation of the phenomena of disease.

In this connection I should like to quote a paragraph by Slesinger[2] in a recent article entitled "The Drift of the Social Sciences."

> Social science shares with medical science the necessity of having to free itself of the desire to do good and of

[1] Presidential address, American Society for Clinical Investigation, Atlantic City, May 4, 1931.

[2] *Survey Graphic*, 19: 24, 1931.

measuring its success by the amount of good accomplished. The medical sciences are only recently beginning to abandon the therapeutic aim in research. It is not to be wondered at, therefore, that the younger social scientists find themselves still slightly hampered by an attitude growing not out of this subject matter, but out of the personnel attracted to the field during a stage in the development of research when welfare was more important than truth.

It is sometimes well to see ourselves as others see us. Let us be sure of our own position in medicine before casting the first stone, as we are perhaps too prone to do, at our younger brothers struggling for a place in the field of science.

Without further delay, let us now turn to a brief consideration of the meaning, for clinical investigation, of the phrase, already quoted, "by the methods of the natural sciences." At the risk of stressing the obvious, let me make it clear at once that I conceive this phrase to refer, not to the techniques or tools used, but to the methods of approach employed in the natural sciences. There are, of course, two methods—on the one hand, observation, analysis and deduction, the so-called descriptive method, still held honorable by some because of its antiquity if for no other reason; on the other hand, the inductive, experimental method, held, it would appear, in higher esteem by most, perhaps because of its relative youth and vigor. To show that this is so, at least among many of those who are devotees of the biological sciences, one need only quote the following from a recent paper on "The Rise of the Experimental Method." Referring to Vesalius on the structure of the human body, and Copernicus on the arrangement of the universe, works which appeared in 1543, the author[3] says, "Intellectual activity of this order had not been witnessed since the days of Aristotle, and one might add that these two books represent the last of the really great achievements of the Aristotelian method in science."

To argue concerning the relative superiority of one method over the other would appear to me to be a somewhat fruitless pastime so far as clinical research is concerned; to hold that the experimental method is necessarily superior to the observational and descriptive method is to hold a narrow and partial view. Both methods are merely complementary halves of the whole, a view so well expressed by Francis Bacon in the "Advancement of Learning," when he says, "All true and fruitful natural philosophy hath a double scale or ladder, ascendent and descendent, ascending from experiments to the invention of causes, and descending from causes to the invention of new experiments; therefore I judge it most requisite that these two parts be severally considered and handled."

[3] Fulton, *Yale Jour. Biol. and Med.*, 3: 299, 1931.

Whether one begins by ascent of the ladder and then descends or reverses the process and begins by descent and then ascends seems to me immaterial, and should depend in each particular case upon the nature of the subject under investigation. What really matters is that he goes both ways and particularly that he stops to think when he reaches the ends of the ladder, whether top or bottom. Are we too busily engaged at present in scrambling up and down the ladder, too little engaged in pausing to think at the top or at the bottom? Again, I merely ask the question and leave you to ponder over the answer.

I do not propose, then, to ask to what extent experimentation is replacing observation, the inductive method, the deductive method in the field of clinical research, but rather, what is the place of each in clinical investigation. To my mind at least, disease is the inductive experiment of Nature, random though it may be, which the investigator in the field of clinical medicine must observe and describe in all its particulars, if he is to develop rational hypotheses to test by experiment. Furthermore, whether he uses merely his natural senses in making observations or turns to the microscope, the test-tube or the balance, appears to me to make little difference, provided he selects the tools most appropriate for the particular problem he is studying. Are the labor and tools of the cabinet maker, because of their precision, superior to those of the artist? I doubt if you will think so. Observation of natural phenomena, analysis and deduction, the descent of the ladder, appears to me to be the first step in clinical investigation and will, I believe, continue and rightly, to engage our time and attention, and perhaps a considerable part of it, certainly for many years to come.

What, then, is the place of the inductive experiment, the ascent of the ladder in clinical investigation? Its value as a method of approach needs no argument to support it. Its application, however, is infinitely more difficult because the subject of experiment is man. On occasion it is possible, when the procedures employed are harmless or when willing and often courageous volunteers offer themselves as subjects for experimentation. Otherwise, the clinical investigator must have recourse to animals to test his hypotheses, must go to the laboratory and become temporarily a pathologist or bacteriologist, a chemist or physiologist. That he is increasingly able to do so is all to his credit, but when he does so let him remember that he has temporarily abdicated his position as a clinical investigator, and that if clinical investigation is to profit he must return to the more difficult problems of the clinic to test out there the hypotheses that he has in turn developed during his sojourn in the laboratory.

Finally, let us turn for a brief moment to a consideration of what changes, if any, are taking place in the direction and scope of clinical research. The historical background has been presented in so masterful a way by Faber[4] that I would not have the temerity to discuss it, even if time permitted. Furthermore, I shall have to limit myself to one field—etiology. Only let me recall that shortly before the founding of this society, under the influence of the rapid advances in bacteriology in the latter part of the nineteenth century, the search for the specific causes of disease was perhaps the most dominant and fruitful activity of the time. In the intervening period it has become more and more apparent to the clinical investigator, a fact of course long recognized in physiology through the influence of Claude Bernard, that the search for specific causes, whether they be living organisms, chemical substances, deficiencies or what not, is but one aspect of a many-sided picture and that a real comprehension of the etiology of disease resides in an understanding of all the conditions or circumstances under which it develops. Here we are concerned with pathogenesis, not etiology in the conventional usage of the word; the study of the interplay of specific agents, environmental factors, and human susceptibilities. Examples are superfluous. To have discovered the tubercle bacillus, its biological characteristics and chemical constitution, does not explain tuberculosis; to have learned the environmental factors which favor its occurrence, still leaves something wanting. The characteristics, susceptibilities and reactions of the host must also be known. The interrelationships of these three factors, and perhaps others, must be studied before one can arrive at an adequate understanding of the pathogenesis of the disease, a point of view so ably illustrated by the studies of Opie[5] in this field. I have cited but one example. Numerous others will occur to you, not only in the field of infectious diseases, but also in the trend of current investigations in diseases of the heart, the blood, the endocrine glands and so on.

Not only is this principle, this attempt to understand all the factors involved and their interrelationships, found in the work of those who are interested primarily in the pathogenesis of disease, but obviously also in the studies of those who are concerned with the phenomena of already existing disease. Consequently, I will not stop to bother you with further illustrations, but will venture to close with the doubtless rash prophecy that in this change in the direction and this enlargement in the scope of clinical investigations lies the evidence that clinical medicine is rapidly approaching a maturity of thought which has characterized the other biological sciences for a longer period of time.

OBITUARY

PROFESSOR SOLON I. BAILEY

DR. SOLON IRVING BAILEY, Phillips professor, emeritus, in Harvard University, died at his summer home in Norwell, Massachusetts, on June 5. His "History and Work of the Harvard Observatory," on which he had been engaged since his retirement in 1925, was published during the week of his death. The final instalment of his "Peruvian Meteorology" is now in press. He had indeed satisfactorily rounded off an industrious and useful scientific life; but although in his seventy-seventh year, he had not yet resigned his interest in the subject of variable stars in globular clusters—a field in which his most notable scientific contributions lie. Within the past two months, with the "History" and the "Meteorology" completed, he resumed his study of the periods of variable stars in the southern globular clusters, planning to devote a year to measurement and computation.

Professor Bailey was born at Lisbon, New Hampshire, December 29, 1854. He received the degree of A.B. from Boston University in 1881, M.A. from Harvard in 1888, and Sc.D. (honorary) from San Augustin University in Arequipa, Peru, in 1923. He became a member of the staff of the Harvard Observatory in 1887, being appointed assistant professor in 1898 and Phillips professor of astronomy in 1912. He was acting director of the Harvard Observatory from 1919 to 1921. His academic distinctions include membership in the National Academy of Sciences, the American Academy of Arts and Sciences, the Astronomische Gesellschaft and the Royal Astronomical Society. He is survived by a widow, Mrs. Ruth E. Bailey, and a son, Professor Irving W. Bailey, of Harvard University.

It is difficult in a short notice to give a fair account of the important services rendered by Professor Bailey to the Harvard Observatory and to astronomy. For three decades he was E. C. Pickering's closest associate in the development of the Harvard Observatory. In two particular instances Bailey guided the work of the present director: he pointed out in 1914 the importance of using the Mount Wilson reflectors for the problems of globular star clusters, and in 1921 he assisted, in a remarkably sympathetic, self-

[4] "Nosography in Modern Internal Medicine," New York, 1923.

[5] "The Harvey Lectures," 197, 1928–29.

effacing and helpful manner, in the inauguration of a new scientific policy in an old and complex observatory. He was largely responsible for the Boyden Station of the Harvard Observatory, having established the Peruvian station in 1890 and explored the possibilities of the South African plateau in 1908 and 1909.

Professor Bailey's early work on the variable stars in globular clusters led to the similar work on the Magellanic Clouds by Miss Leavitt. He devoted more than twenty years to the study of variable stars in star clusters, producing four monographs on the subject. The work also involved the classification of star clusters and the study of stellar distribution within the brighter systems.

Bailey was a pioneer in the photographic discovery and measurement of extra-galactic nebulae. With his associates he added several thousand new systems to our catalogues, the work being based almost exclusively on the photographs made with the Bruce refractor at the southern station. Incidentally, Bailey has long had the reputation of having made the best photographs obtained with that important but somewhat rebellious instrument.

When in 1922 at the age of sixty-seven Bailey returned to Arequipa to take charge of and rehabilitate the southern station, he resumed his studies of globular clusters, which had been interrupted by some years of administrative work. He also turned his attention to stellar distribution and made extensive star counts on long exposure Bruce photographs covering the south galactic pole and the rich regions of the southern Milky Way. This work has been much quoted in recent years.

Next to his work on globular star clusters, Bailey's volume on the "History and Work of the Harvard Observatory" will be most remembered in future years. His long association with the observatory made him the logical person to survey the development and the scientific problems of one of the oldest of American research institutions. This new volume is divided into three parts, the first dealing with the historical background and material growth of the observatory; the second discussing briefly the numerous research problems of the past and present, and the third dealing biographically with individuals on the observatory staff. In the second part he discusses the observatory's contributions to the problems of the solar system, the astronomy of position, astronomical photography, stellar photometry, spectroscopy, variable stars and novae, clusters and nebulae, and the structure and dimensions of stellar systems. He approaches personal problems with kind generosity, and scientific problems, especially the newer developments, with conservatism and objectivity. Such an attitude was characteristic of him in all his dealings with people and problems; it was the source of his high standing throughout the past forty-five years in the regard of the observatory staff and of the general astronomical community.

HARLOW SHAPLEY
HARVARD COLLEGE OBSERVATORY

RECENT DEATHS

DR. STEPHEN MOULTON BABCOCK, emeritus professor of agricultural chemistry at the University of Wisconsin, died on July 1. He was eighty-eight years old.

DR. GEORGE FILLMORE SWAIN, Gordon McKay professor of civil engineering at Harvard University, died suddenly on July 1 in his seventy-fourth year.

PROFESSOR MYER EDWARD JAFFA, professor of nutrition emeritus in the University of California, chief of the Bureau of Food and Drugs of the California State Board of Health since 1925 and a consulting nutrition expert for the board since 1915, died on June 28 at the age of seventy-three years.

DR. CHARLES ALLEN PORTER, professor emeritus of clinical surgery at the Harvard Medical School and formerly surgeon-in-chief of the Massachusetts General Hospital, died on July 3 in his sixty-fifth year.

DR. ALBERT E. STERNE, professor of nervous and mental diseases at the Indiana University School of Medicine, died on June 30 at the age of sixty-five years.

JOHN EDWIN STARR, president of the Starr Engineering Company of New York, a former president of the American Society of Refrigerating Engineers, has died at the age of seventy-one years.

PROFESSOR HARALD HÖFFDING, who held the chair of philosophy in the University of Copenhagen from 1883 to 1915, and was distinguished for his contributions to psychology, died on July 2. He was eighty-eight years old.

THE death is announced of M. E. Cossarat, director of the observatory at Toulouse.

SCIENTIFIC EVENTS

THE BRITISH NATIONAL PHYSICAL LABORATORY

THE National Physical Laboratory at Teddington was open yesterday afternoon to visitors, and the annual function was preceded by the formal opening of the new physics building. This building will eventually form three sides of a rectangle, but only the central part has so far been erected. Its door

was unlocked by Sir Gowland Hopkins, president of the Royal Society, and chairman of the general board of the National Physical Laboratory, with a gold key, handed to him by the architect, Mr. F. A. Llewellyn, after speeches by Sir Richard Glazebrook, the first director of the laboratory, and Sir Joseph Thomson.

Sir Joseph Thomson congratulated Sir Richard Glazebrook on this further step in the phenomenal development of the National Physical Laboratory, which owed its prosperity and progress to his wisdom, energy and insight more than to anything else. The National Physical Laboratory had a great part to play in the modern applications of science to industry.

The completed portion of the new physics building will house the heat and general physics section of the physics department, with part of the radiology and sound work. Apparatus for the measurement of noises was on view in this building. The loudness of the noise to be assessed is determined by varying the strength of a standard sound until it is either just "drowned" by the noise or judged to be equally loud. It was explained that, measured on a convenient scale of loudness with zero at the threshold of hearing, sounds become painful at about 130 "degrees" above threshold, where each "degree," known as a decibel, is approximately the least change in loudness perceptible to the ear. The level of conversational speech is at about 50–60 decibels above threshold. Aeroplane cabin noises are at present in the region of 80–110 decibels above the threshold. The lower of these levels corresponds to that of the noise in a tube train, and it is almost impossible to converse in a loudness level of 110 decibels, even by shouting. In street traffic and in ordinary trains the noise ranges from 50–70 decibels above threshold, and the laboratory is assisting the Aeronautical Research Committee in its endeavor to reduce noise in aircraft cabins to such a level.

One of the new exhibits in the Aerodynamics Department this year was a large steel tunnel, the equipment of which is nearly completed, in which compressed air at 25 atmospheres pressure can be circulated round an aeroplane model. Tests carried out under this high pressure are directly comparable with those on a full-scale machine—in other words, the "scale effect" which exists when working with a model in a tunnel at atmospheric pressure is eliminated. When the compressed air is circulated at full pressure by an airscrew the conditions will correspond with those surrounding a full-sized aeroplane flying at 150 miles an hour. In one of the older wind tunnels tests were shown in progress on a model of the Hill Pterodactyl, the new tailless aeroplane. A new and neat method of rendering streamline flow visible was displayed in the same department. A number of fine platinum wires, heated by an electric current, are stretched across the airflow near a model. Each wire gives rise to a band of heated air, which follows the direction of the streamline passing the wire. The shadows of these heated air filaments are cast on a screen and thus give a direct picture of the streamlines.

THE POST-GRADUATE MEDICAL SCHOOL AND HOSPITAL AND COLUMBIA UNIVERSITY

IN accordance with the agreement between the New York Post-Graduate Medical School and Hospital and Columbia University, effective on July 1, by which the former became the Post-Graduate School of Medicine of Columbia, an administrative board of post-graduate studies in medicine has been established on which will be represented members of the governing body of the university, the undergraduate medical school and the postgraduate school of medicine.

Under the terms of the affiliation, this board will have general oversight and control of all post-graduate instruction in medicine offered by the university, whether at the Medical Center, the Post-Graduate Medical School or elsewhere in the city, and is constituted as follows: Dean Willard C. Rappleye, *chairman,* Dr. Linsly R. Williams, Dean Howard Lee McBain, Director James C. Egbert, Dr. Walter W. Palmer, Dr. James W. Jobling, Dr. Frederick Tilney, Dr. Arthur F. Chace, Dr. Herman O. Mosenthal, Dr. Howard F. Shattuck, Dr. Edward H. Hume, Dr. Harry S. Dunning, Dr. Lewis F. Frissell and Mr. Frank D. Fackenthal.

Dr. Edward H. Hume has been appointed director and Dr. Alan R. Anderson associate director of the New York Post-Graduate Medical School.

This incorporation of the New York Post-Graduate Medical School into the teaching system of Columbia University as its post-graduate school of medicine, distinct from the undergraduate school, carries into fruition, after some forty-nine years, the ambition of the seven founders of the Post-Graduate who resigned from the faculty of the New York University upon the refusal of the trustees to grant them a separate building for post-graduate instruction and, early in the year 1882, organized the present New York Post-Graduate Medical School and Hospital.

Dr. D. B. St. John Roosa, first president of the institution he helped to found, in his inaugural address in November, 1882, pointed out that while temporarily undertaken in an independent institution, this type of medical education rightly belonged with other forms of professional instruction within a uni-

versity, and closed with the forecast, "the time will come when this institution will be included within a great university."

DR. SWASEY'S GIFT TO THE ENGINEERING FOUNDATION

AT a dinner on June 31 at University Club, New York City, given by the chairman, H. Hobart Porter, of the Engineering Foundation, President John V. N. Dorr, of the United Engineering Trustees, Incorporated, and Mr. Porter announced that Dr. Ambrose Swasey, founder of the Engineering Foundation, had added $250,000 to his previous gifts, bringing their total to three quarters of a million dollars.

In making this gift Dr. Swasey said: "For many years past, and especially since 1914, I have thought that a great service to mankind might be rendered by the stimulation of research and development work in the engineering profession, and have sought to further that end by assisting in establishing the Engineering Foundation, whose income would be available for such purposes. It has been my privilege on previous occasions to assist in the endowment of this foundation, and, having a high appreciation of the very practical and helpful results that have been already achieved, and believing that the broad and well-laid plans of the foundation promise even greater service in the future, I now take great pleasure in making available to the Engineering Foundation, through United Engineering Trustees, Inc., an additional fund of $250,000 for the furtherance of research in science and engineering, or for the advancement in any other manner of the profession of engineering and the good of mankind."

Mr. Swasey was born at Exeter, New Hampshire, nearly eighty-five years ago. He is the surviving member of the firm of Warner & Swasey, of Cleveland, Ohio, famous for the building of great telescopes, and of instruments and machine tools of precision. The Engineering Foundation was founded by the national societies of Civil, Mining and Metallurgical, Mechanical and Electrical Engineers in 1914 on the basis of his conception of a research instrumentality for the profession of engineering and for broad services to mankind.

There were present more than thirty presidents and former presidents of national engineering societies, of the United Engineering Trustees and of the Engineering Foundation, and other nationally prominent engineers. The guests were:

L. H. Baekeland, former president, American Institute of Chemical Engineers; H. F. Bain, secretary, Mining Engineers; Philip E. Bliss, president, Warner and Swasey Company; H. P. Charlesworth, trustee, United Engineering Trustees; G. H. Clevenger, vice-chairman, Engineering Foundation; H. V. Coes, trustee, United Engineering Trustees; J. Vipond Davies, past president, United Engineering Trustees; J. V. N. Dorr, president, United Engineering Trustees; Gano Dunn, past president, Electrical Engineers and United Engineering Trustees, and first chairman, Engineering Foundation; A. S. Dwight, past president, Mining Engineers; W. S. Finlay, Jr., former member, Engineering Foundation; Alfred D. Flinn, director, Engineering Foundation; Bancroft Gherardi, past president, Electrical Engineers and United Engineering Trustees; A. M. Greene, Jr., past-president, Society for the Promotion of Engineering Education; O. E. Hovey, vice-chairman, Engineering Foundation; C. T. Hutchinson, former secretary, Engineering Foundation; D. S. Jacobus, past-president, Mechanical Engineers; F. B. Jewett, past-president, Electrical Engineers and former vice-chairman of Engineering Foundation; H. A. Kidder, vice-president, United Engineering Trustees; G. L. Knight, trustee, United Engineering Trustees; H. A. Lardner, ex-president, New York Electrical Society; W. S. Lee, president, Electrical Engineers; Geo. A. Orrok, former vice-chairman, Engineering Foundation; G. H. Pegram, past-president, Civil Engineers; H. Hobart Porter, chairman, Engineering Foundation; W. E. Reed, formerly of Warner and Swasey Company; Robert Ridgway, past-president, Civil Engineers; Charles F. Scott, past-president, Electrical Engineers; C. E. Skinner, president-elect, Electrical Engineers; J. Waldo Smith, former vice-chairman, Engineering Foundation; L. B. Stillwell, former chairman, Engineering Foundation, and past-president, Electrical Engineers; Ambrose Swasey, founder Engineering Foundation, past-president, Mechanical Engineers, honorary member, Civil Engineers; Calvert Townley, past-president, Electrical Engineers; A. L. Walker, former member, Engineering Foundation; Roy V. Wright, president, Mechanical Engineers.

THE CENTENARY OF THE HARVEIAN SOCIETY OF LONDON

ACCORDING to an article in *The British Medical Journal* the celebration of the centenary of the Harveian Society of London began with a largely attended meeting at St. Bartholomew's Hospital on the afternoon of June 11, presided over by the president of the society, Sir Thomas Horder. Dr. Raymond Crawfurd delivered the address on "The Place of Medical Societies in the Progress of Medicine," after which Sir Humphry Rolleston proposed a vote of thanks. Sir Thomas Horder then presented medals, specially struck for the centenary to Dr. D. Elliot Dickson, representing the Harveian Society of Edinburgh, and to Dr. W. H. Welch, professor of the history of medicine at the Johns Hopkins University, representing the Harveian Society of New York.

Dr. Dickson, in returning thanks, said that he was proud to represent the Edinburgh society, and also

proud as a Scotsman to have heard Dr. Crawfurd's reference to the Royal Medical Society of Edinburgh. Professor Welch said that all members of the New York Society would heartily appreciate the generous words in which Dr. Crawfurd had referred to its activities. The principal function of that society was in its lectureships, perhaps six or eight lectures being delivered during the year in the general field of scientific medicine, and it was obvious that there was no name in history which could be more appropriately used as the sponsor of such a society than that of William Harvey. Dr. Welch handed to the president an illuminated scroll on behalf of the New York Harveian Society, inscribed with cordial greetings on the occasion of the hundredth anniversary, and expressing appreciation of the significance of continuous corporate existence for so long a period: "An achievement so signal bears witness to the serviceableness to its members of any association. In medicine it bears evidence furthermore, to the success of its function in the maintenance of a high standard of scholarship and its usefulness in the dissemination of learning."

The society's centenary dinner was given by Mr. George Buckston Browne in the Hall of the Grocers Company of the City of London. After the loyal toasts, the president welcomed Prince Arthur of Connaught as a newly admitted honorary member of the society, and presented to him a commemorative bronze medal. His Royal Highness then proposed the toast of "Prosperity to the Harveian Society of London." He felt it a great—though perhaps an "irregular"—honor to be admitted to membership of the society, in company with Professor Welch, the doyen of the medical profession of the United States. The health of the visitors was proposed by Sir StClair Thomson. This feast, he said, gave an opportunity for the mingling of men of different occupations, as well as of men working in different branches of the same profession. Harvey, a man of wide humanity and culture, would have welcomed this occasion, for had he not exhorted his brethren to "dwell together in loving friendship"? Professor Welch, *The British Medical Journal* says, made in response a charming and spirited speech of appreciation for the way in which "such things are done in London." He regarded centenary celebrations as very valuable and interesting things, which appealed to him particularly as contributions to the history of medicine. Lord Dawson, who also replied, spoke of Professor Welch as a citizen of the world, exemplifying the unity of purpose of the English-speaking medical profession. He himself, during his visit to Canada and the United States last year to attend the annual meeting of the British Medical Association in Winnipeg, had observed the inexhaustible energy and enthusiasm of their octogenarian guest. On behalf of the Royal College of Physicians, with its long and intimate associations with Harvey, Lord Dawson congratulated the society on attaining its hundredth year.

The final event of the celebrations took the form of a pilgrimage to the tomb of William Harvey at Hempstead Church in Essex, near Saffron Walden. A party of about sixty, headed by Sir Thomas Horder, Sir D'Arcy Power, Sir StClair Thomson, Dr. Herbert Spencer and Professor Welch, journeyed by road from London on the morning of June 13. At Hempstead Church a brief service was held, and the Bishop of Colchester gave an address.

SCIENTIFIC NOTES AND NEWS

THE doctorate of science was conferred on June 15 by the University of Rochester on Dr. Harvey Cushing, Moseley professor of surgery at the Harvard Medical School and surgeon-in-chief at the Peter Bent Brigham Hospital.

AT the commencement exercises of Purdue University, the honorary degree of doctor of science was conferred on Dr. J. C. Arthur, professor emeritus of botany; on Dr. Stanley Coulter, professor emeritus of biology, and on Professor H. A. Huston, consulting agricultural chemist, Kew Gardens, New York, who was connected with Purdue from 1884 to 1903, the last year as director of the Agricultural Experiment Station.

THE University of Michigan conferred at commencement the degree of doctor of science on Dr. Warren Plimpton Lombard, for thirty-one years professor of physiology in the university medical school, professor emeritus since 1923. The doctorate of engineering was given to Clarence E. Grosbeck, of the class of 1898, prominent in public utilities, and on William Aiken Starrett, of the class of 1897, designer and builder of the Empire State Building in New York City.

MR. A. C. FIELDNER, chief engineer of the Experiment Stations Division, United States Bureau of Mines, Washington, D. C., was awarded the Lamme Meritorious Achievement Medal by the Ohio State University on June 8. This gold medal is awarded annually to a graduate of one of the departments of the university for meritorious achievement in engineering or the technical arts.

THE Monaco prize of 100,000 francs, established by Prince Albert of Monaco and awarded every two

years by the Paris Academy of Medicine to aid some French scientific man in his researches, has been given to M. Veillon, of the Pasteur Institute, in recognition of his work in bacteriology.

DR. L. J. COLE, professor of genetics at the University of Wisconsin, was recently elected a corresponding member of the Czechoslovak Academy of Agriculture.

A DINNER in honor of Dr. Alexander G. McAdie, who has retired as A. Lawrence Rotch professor of meteorology at Harvard University and as director of the Blue Hill Observatory, was given recently by the members of the Harvard Overseers' Committee appointed to visit the observatory.

A PORTRAIT of Dr. John M. Fisher, associate professor of gynecology at Jefferson Medical College, Philadelphia, and oldest ex-intern of Jefferson Hospital, was presented to the college on June 3. It was the gift of the alumni of the college. Dr. Frank C. Hammond made the presentation, and Dr. Ross V. Patterson, dean of the college, accepted it.

MR. J. D. FIGGINS completes his twenty-first year as director of the Colorado Museum of Natural History on July 15. Mr. Charles H. Hanington, president, and Mr. Walter C. Mead, vice-president, on behalf of the board of trustees, have expressed their appreciation for his long service and faithful work in building up the museum. They say: "Coming to us from the American Museum, New York City, when we were a new institution, it was through his untiring efforts that we have grown into an organization of nation-wide interest."

OFFICERS of the American Society of Clinical Pathology have been elected as follows: Dr. W. M. Simpson, director of clinical laboratories, Miami Valley General Hospital, Dayton, Ohio, *President-elect;* Dr. C. J. Bucher, department of pathology, Jefferson Medical College, Philadelphia, Pennsylvania, *Vice-president.* The following were elected as new members of the executive committee: Dr. K. M. Lynch (retiring president), professor of pathology, Medical School of the State of South Carolina, Charleston; Dr. A. G. Foord, Pasadena Hospital, Calif. The Ward Burdick award was made to Dr. W. G. Exton.

DR. JAMES R. CASH, since 1924 professor of pathology at the Peiping Union Medical College, China, has been appointed to the Walter Reed professorship of pathology at the University of Virginia, succeeding the late Dr. Harry T. Marshall as the second incumbent of the chair.

PROFESSOR G. H. HARDY, Savilian professor of geometry at the University of Oxford, has been elected to the Sadleirian professorship of pure mathematics, in succession to Professor E. W. Hobson, who has resigned.

THE General Board of the University of Cambridge was authorized on June 12 to reappoint Sir Horace Lamb, of Trinity College, to the Rayleigh lectureship in mathematics in the faculty of mathematics, without stipend, so long as he shall desire to hold that office.

DR. T. THOMSON FLYNN has been appointed to the chair of zoology in the University of Belfast. Dr. Flynn is a doctor of science of the University of Sydney, and is at present Ralston professor of biology in the University of Tasmania.

DR. FRITZ VON WETTSTEIN, of the University of Göttingen, has been called to Munich.

DR. CESARI FRUGONI, for nine years professor of special medical pathology at Florence, has been appointed professor of clinical medicine at Rome.

AT the recent annual meeting of the board of trustees of the Tropical Plant Research Foundation, Dr. William Crocker, director of the Boyce Thompson Institute of Yonkers, New York, was elected acting director and general manager of the foundation. It was decided to move the offices to Yonkers during a temporary period. Until further notice all mail to the foundation should be addressed to 1086 North Broadway, Yonkers, New York.

THE U. S. Department of Agriculture has announced the following appointments to its research staff: Dr. Edward Maris Harvey, formerly professor of horticultural research at the Oregon State Agricultural College, physiologist in the division of horticultural crops and diseases of the Bureau of Plant Industry, stationed at Pomona, California; Dr. Fisk Gerhardt, formerly assistant chemist at the Iowa Agricultural Experiment Station, physiologist, stationed at Wenatchee, Washington; Mr. Henry Hartman, formerly professor of pomology at the Oregon State Agricultural College, horticulturist in the Division of Horticultural Crops and Diseases of the bureau, stationed at Wenatchee, Washington.

MR. R. W. HARNED, of the State Plant Board of Mississippi, has been appointed leader of the division of cotton insect investigations of the Bureau of Entomology to fill the vacancy caused by the resignation of Mr. B. R. Coad.

AT the recent annual meeting of the American Association of Cereal Chemists at Louisville, Dr. D. A. Coleman, marketing specialist of the Bureau of Agricultural Economics of the U. S. Department of Agriculture, was made editor of *Cereal Chemistry,* the official organ of the association.

WE learn from the *Journal* of the American Medical Association that Dr. Edward N. Brush, associate editor of the *American Journal of Psychiatry* for many years and editor since 1904, presented his resignation during the annual meeting of the Amer-

ican Psychiatric Association on June 4, in Toronto. His successor, Dr. Clarence B. Farrar, formerly of Baltimore and now of Toronto, was introduced at a dinner for Dr. Brush, when an illuminated parchment and a purse of $1,200 were presented to him.

DR. RALPH H. CHENEY, professor of biology, Long Island University, has been made resident investigator (economic plants) at the Brooklyn Botanic Garden beginning on July 1. The Botanic Garden has recently entered into an agreement with Long Island University for cooperation with the university's department of biology. Courses given at the Botanic Garden which conform to academic standards approved by Long Island University will be accepted by the university for undergraduate credit. As the university has not yet established a graduate school, it does not confer credits leading toward a graduate degree. The professor of botany of the university will act in an advisory capacity to the Botanic Garden whenever called upon to do so.

MR. M. J. SEAVY has been elected president of the Palo Company. Mr. Seavy has been connected with the company for fifteen years and has been vice-president and manager for the last eight years. It is planned to enlarge the facilities of the company for furnishing laboratory supplies.

THE National Research Council will be represented at the meeting of the General Assembly of the International Research Council in Brussels on July 11 by three delegates: Dr. John C. Merriam, president of the Carnegie Institution of Washington, D. C.; Dr. A. E. Kennelly, professor of electrical engineering, emeritus, Harvard University, and Dr. Frank Schlesinger, professor of astronomy and director of the Observatory of Yale University. Dr. Schlesinger was a member of the committee of fifteen appointed two years ago by the International Research Council to draft a revision of the statutes of the council.

DESIGNATION of American delegates to the sixth International Congress on Industrial Accidents and Diseases, to be held at Geneva from August 3 to 8, under the auspices of the Swiss Federal Council, has been approved by President Hoover. The American delegation includes: Dr. Francis D. Patterson, of Philadelphia, chief surgeon of the Pennsylvania Railroad, specialist in industrial hygiene; Dr. Fred H. Albee, professor of orthopedic surgery in the College of Physicians and Surgeons of Columbia University; Dr. Francis D. Donoghue, of Boston, medical adviser of the Department of Industrial Accidents of the State of Massachusetts; Dr. Emery R. Hayhurst, professor of hygiene in the College of Medicine of the Ohio State University, and Dr. Emma F. Ward, of Baltimore, research investigator attached to the office of industrial hygiene and sanitation in the United State Public Health Service.

DR. CHARLES J. FISH, director of the Buffalo Museum of Science, has accepted charge of an international survey to determine the effect on the herring industry of the proposed power dam at Passamaquoddy Bay, Maine. Dr. Fish has received a leave of absence from the board of managers of the museum and will go on July 10 to the Canadian biological laboratory at St. Andrews, N.B., the seat of the two-year investigation. Other members of the commission are: Dr. A. G. Huntsman, director of the Atlantic Biological Station, Canada; O. E. Sette, in charge of North Atlantic investigations for the U. S. Bureau of Fisheries; W. A. Found, deputy minister of fisheries, Canada, and Dr. H. B. Bigelow, director of the Woods Hole Oceanographic Institution. Dr. Fish will be executive secretary of the commission and will have charge of the work in the field.

DR. FRANCIS R. FRASER has accepted an invitation to give the third series of the Abraham Flexner Lectures during the year 1932–1933 at Vanderbilt University. Dr. Fraser is director of the medical clinic and professor of medicine in the St. Bartholomew's Medical School and Hospital in London. He is a graduate in medicine of the University of Edinburgh, and spent some years in New York City at the Rockefeller Institute and the Presbyterian Hospital. Dr. Fraser recently filled an appointment as a special representative from England to confer with the medical faculties of Australia. While there he gave lectures and conducted clinics.

THE American Phytopathological Society will hold its third annual summer tour and conference from July 28 to 31, under the immediate direction of Drs. H. W. Anderson, Leslie Pierce, M. W. Gardner and C. T. Gregory. For those not driving cars will be furnished. Dr. H. W. Anderson, Illinois Agricultural Experiment Station, Urbana, should be addressed for further particulars and arrangements.

THE ninety-ninth annual meeting of the British Medical Association will be held at Eastbourne, from July 21 to 24, under the presidency of Dr. William G. Willoughby, Eastbourne.

WE learn from the *Harvard Alumni Bulletin* that the late Carroll Everett Edson, '88, A.M. and M.D. '92, just before his death prepared a will leaving the remainder of his residuary estate, after the death of certain life tenants and annuitants, to Harvard College for research in climatology, expressing the hope that the fund might be used for the establishment of a professorship under the name of the Edson Professorship of Climatology. As a sudden stroke pre-

vented the execution of this will, his sisters have executed an indenture of trust to carry out their brother's purpose.

THE *Journal* of the American Medical Association reports that the Leonard Wood Memorial Committee has completed its goal of a $2,000,000 endowment for the eradication of leprosy. The campaign was started in 1927 on the appeal of the late General Wood, then Governor-General of the Philippines, and was carried on after his death as a memorial to him. The actual amount received was $2,031,000, contributions being received from 50,000 persons.

AN Associated Press dispatch reports that the California State Park Commission has announced the successful completion of negotiations for purchase of additional forest lands for the park system with receipt of a check for $1,000,000 from Mr. John D. Rockefeller. Ten thousand acres of redwoods will be added to state parks. The campaign was sponsored over a ten-year period by the Save-the-Redwoods League, of which Dr. John C. Merriam, president of the Carnegie Institution of Washington, is the head. Mr. Rockefeller not only gave a million dollars unconditionally but pledged a second million to match private gifts as received. The sum to be matched had reached between $600,000 and $700,000.

THE *Harvard Alumni Bulletin* reports that the largest telescope in the eastern part of the country, a 60-inch reflector, has been secured by the Harvard Observatory and will be placed in eastern Massachusetts at a site not yet chosen, probably within 25 miles of Boston. The new telescope will be of the reflector type, similar in size and general structural features to the 60-inch telescope now being installed at the southern station of the Harvard Observatory in Bloemfontein, South Africa. Accessory equipment will include apparatus for the study of spectra and light variations of the stars, their temperatures, dimensions and motions. The Harvard Observatory equipment has heretofore been specially suited to covering fields of a large number of stars; the new 60-inch reflector will specialize in the closer analyses of individual stars and planets.

SIXTY geologists from various parts of Pennsylvania met at the Pennsylvania State College from May 29 to 31 for the purpose of studying the geology of Central Pennsylvania. The Paleozoic stratigraphic section from the Cambrian to the Pennsylvanian and the geologic structure of the region were observed on a field trip from Gallitzen to Altoona and Tyrone. The Oriskany gas horizon was also studied along its outcrop and a visit was made to the Bell Mine of the American Lime and Stone Company at Bellefonte. Optional trips were also scheduled to the various caves of the region. At an evening meeting it was decided to form a permanent organization for the purpose of conducting similar trips in other parts of the state. The following committee on organization was appointed: Dr. B. L. Miller, Lehigh University; Mr. R. W. Clark, of the Gulf Companies, and Dr. C. A. Bonine, Pennsylvania State College, *chairman*. The next conference will be held in the Lehigh Valley region under the direction of the Lehigh University and Lafayette College departments of geology.

SCIENCE SERVICE reports that a party of sixteen University of Chicago students under the supervision of Dr. Fay-Cooper Cole has begun excavation at Indian mounds near Lewistown, Illinois, in the hope of discovering information about the oldest known inhabitants of the region. These most ancient inhabitants have been named the "black sand" people. The name was given them because nine skeletons were found buried in black glacial sand beneath Indian mounds last summer. This year the expedition hopes to recover implements and ornaments which will shed light on the home life and customs of the tribe. The black sand Indians are estimated to have lived at least 2,000 years ago. Pointing out that the search for early inhabitants of the Mississippi Valley is highly important in solving problems of American prehistory, Dr. Cole stated that Fulton County, where the excavations are being made, contains "the most complete data for culture sequence yet found in the Mississippi Valley."

FORTY-SEVEN states and territories are now extending financial support to state forestry, most of them having state foresters and participating in fire control, extension, reforestation or other related activities, according to the Forest Service. State forestry appropriations for the last year reached a total of $7,297,935. Of this amount, $2,555,329 was appropriated for fire protection by 38 states in cooperation with private landowners and the Federal Government. A total of $1,106,711 was used by 40 states for growing tree-planting stock and for reforestation. More than $2,308,000 was appropriated for purchase, maintenance and improvement of state forest lands. Other appropriations were used to fight pests and tree diseases, and for education, research and extension work. In addition, large private expenditures were made in forestry activities in most of these states. State appropriations for the last two years showed a gain of more than $2,000,000 a year over similar forestry appropriations for 1927 and 1928. Cooperation between the Forest Service and the states was enlarged during the past year. The

Federal Government contributed more than $1,400,000 for protection and reforestation in cooperating states. Cooperative fire protection was extended to several million acres which had not previously been covered. The work of growing and distributing trees for forest planting also was enlarged.

DISCUSSION

CRYSTALLINE AMYLASE

NORTHRUP and Kunitz's recent announcement, in these columns, of the crystallization of trypsin, the pancreatic protease, prompts us to record here the crystallization of pancreatic amylase also.

From buffered alcohol-water solutions of pancreatic amylase, freshly purified by methods previously described from this laboratory, crystals have been obtained which show enzymic activity almost as high as the maximum observed in the highly purified preparations of this enzyme as previously prepared and studied.

The crystalline amylase is obtained as very minute isotropic elongated crystals which exhibit slight double refraction. Professor P. F. Kerr, of our department of mineralogy, to whom we are indebted for the crystallographic examination, reports further that the index of refraction as determined by immersion is approximately 1.54.

In view of the protein or protein-like nature of the crystals and their very slow deposition from the alcoholic systems, it is exceedingly important to work with a very small temperature gradient and in the region of hydrogen-ion activity corresponding to the isoelectric point.

The hydrogen-ion activity of the solution is adjusted by means of phosphate buffers in such concentration that they are not separated out at the temperatures employed. Both formation and yield of crystals are very dependent upon the hydrogen-ion activity of the system.

Careful study and rigorous observance of the best experimental conditions thus far found make it possible to obtain crystals with regularity; but as yet only in very small amounts. Furthermore, the crystals are so minute, so light and so unstable that even after they have been formed they must be handled by means of highly specialized and very time-consuming technique.

Hence it appears inevitable that further progress must be exceedingly slow and it therefore seems best to record, at this preliminary stage of the work, the fact that this much-studied and presumably typical amylolytic enzyme has been obtained in crystalline form, even if as yet only upon a small scale.[1]

M. L. CALDWELL
L. E. BOOHER
H. C. SHERMAN

[1] Contribution No. 661 from the Department of Chemistry, Columbia University, New York.

PSYCHIC ANALOGUES OF ALLERGY

WHEN a foreign protein comes in contact with the tissue cells of a normal organism these become irritated and enter upon a cycle of unwonted metabolic change, the results of which appear in a week or two.

The first of these results is a specific hypersensitiveness toward the foreign protein in question so that thereafter infinitesimal quantities of the latter may be able to irritate the cells.

Experiment has made it obvious that the acquirement of specific sensitization goes hand in hand with the development of extraordinary powers within the tissue cells for rapid destruction, digestion or neutralization of the offensive foreign protein. The effective cellular agents in these reactions we know as *antibodies*. Their varieties and qualifications transcend our knowledge.

While the details of these cellular evolutions are exceedingly complex it is manifest that they are all coordinated to effect a purpose; to protect the body against a foreign poison; to endow it with what we call immunity toward the harmful substance which first irritated the cells.

Study of the pathogenesis of various infections leads irresistibly to the conclusion that, by and large, the essence of all infectious diseases inheres in the foreign proteins liberated in the body through the presence of invading microbes. Obviously no range of human endeavor is more fraught with problems affecting human welfare than that which is concerned with the understanding and control of the poisons leading to immunity.

There are doubtless several ways by which the biological status of immunity may be attained, but only one concerns us here—that, namely, in which the foreign protein or "antigen," as it is called, on entering the normal body has been able in the course of a few days to excite the latter to develop essentially new vital powers objectively witnessed by specific hypersensitiveness and defensive antibody reactions. The normal body which has undergone such a course of cellular training is now said to be "immune" toward that particular antigen, or microbe, which started the disturbance. It is characteristic of the immune state that the body which has acquired it responds at once with defensive reactions calculated to inhibit or destroy the irritating antigen whenever thereafter the latter makes a new invasion.

It might safely be predicted that all future contests

between the assaulted organism and the intruding pathogens must be determined on a merely quantitative basis according to the number and virulence of the latter.

When these factors are small enough the prepared defenders of the body destroy the invaders without perturbation, and gain strength by the exercise; the conscious mind is oblivious of the danger which had threatened its citadel.

But in actual experience or at the will of the experimentalist the number and virulence of the intruding microbes may surpass the abilities of the prepared mechanisms of defense. For the invaded host the contest then concerns, as it were, not the occasional criminal helpless before the bar of justice, but a national enemy threatening subjugation of a whole people. The result can only be determined by a war. In vital statistics this war is classified as "disease"; for the experimentalist its activities are broadly expressed by the word "allergy." Allergy implies a defensive reaction against a particular antigen in a prepared subject. The energy of the conflict is a function of the parity and the powers of the contenders. A decisive ending may signify the victory of either contestant. It may be assumed that in the scheme of Nature there is a definite purpose behind all these cellular changes in an animal which follow its infection with pathogenic microbes and culminate after some days in an allergic state capable of explosive physiological reactions under the stimulus of reinfection. Indeed, as will be indicated in a contribution as yet unpublished, the manifestation of allergy is not saved up like gifts for Christmas, ready for a grand celebration when a set time is reached; on the contrary, there is reason to believe that allergy, which includes specific hypersensitiveness and altered reaction, is initiated in the tissue cells at their earliest intercourse with invading germs. Nevertheless we can not admit that the ultimate purpose of nature is merely to develop in protoplasm a capacity for explosive disintegration. Rather her aim is peace—to be achieved by war only when necessary. And peace is complete immunity.

Nevertheless, the establishment of acquired immunity, either through the accident of disease or the intention of experiment, is commonly bound up with the inflammatory reactions of allergy which, like a club in the hands of a blind ally, may strike friend or foe alike.

The protection of the host through the tissue and functional reactions of allergy is so constant a concomitant of reinfection, or as the French prefer to say, super-infection, that a majority of the leaders in immunology assume that the sword of accomplishment in the establishment of immunity is allergy itself, or that allergy is the indispensable directive force of reactions through which immunity is established.

Calmette,[1] himself a proponent of this view, squarely states the paradox that the same allergic reaction in the host which under favorable conditions holds in check or forthwith destroys the microbic enemies of reinfection may, when the cellular responsiveness is enfeebled, turn upon its master and hasten his dissolution.

The distinguished American spokesman for the same point of view, Allen K. Krause,[2] closes a series of brilliant experimental researches aimed at the identification of allergy as the essential and effective agent of immunity, with the frank confession that, in spite of all demonstrated probabilities in favor of his theme, there still lacks convincing proof that his thesis is sound.

Now come Rich and McCordock[3] who, in a bold and most instructive essay, flatly deny that allergy is the essential basis of immunity. According to them the evidence of facts is decidedly against rather than in favor of such a view. While admitting that allergic reaction may at times favor protection of the host, such a relation is casual and not essential. The nub of their argument consists in the known lack of parallelism between the intensities of allergy and immunity. Either may be conspicuously weak while the other is correspondingly strong.

The whole argument seems to have reached a stalemate. The fast accumulating wealth of immunological facts finds no general law for their classification. There lacks a shibboleth—touchstone of fealty as between cell and microbe.

It has occurred to the writer that our philosophy of biological reaction may need a new point of view, less narrowly mechanistic than we can expect from our laboratories of to-day.

No physical proofs can be offered, only suggestive analogies from the psychic phase of that life of which, it is commonly agreed, allergy and immunity represent quasi-functions of the soma.

Consider pain. How negligible in health and well-being! Yet were there no such sensation it is safe to predict that animal life would not long persist. Consider fear, the warning forerunner of impending evil; the advocate of protective preparation. How universal and all-controlling this emotion and that

[1] A. Calmette, "L'Infection bacillaire de la Tuberculose chez l'homme et chez les animaux," 1920.

[2] A. K. Krause, and Krause with Willis or Peters, Numerous contributions to the *Amer. Rev. of Tuberculosis.*

[3] A. R. Rich and H. C. McCordock, *Bull.* Johns Hopkins Hospital, xliv, 273, 1929.

sensation. These two incidentals of the psychic life would be monstrously out of place in a trouble-free Eden, in our fool-a-day world they unite to make the cornerstone of human activities.

Using words of broad signification, we may assume that through fear the mind keys the body to instant defensive response for mutual protection. But there was not always a mind. It is a late sublimation of protoplasmic function.

But it is inconceivable that communities of living cells should have failed in the essentials of defensive reaction while waiting the development of a psychological organ. The elaboration of a technique of immunity must have kept pace step by step with the lethal powers of poisonous foes, else sooner or later must the defenseless race have succumbed. It seems probable that this technique accomplishes its purpose of immunity through two different methods.

The immigration of limited numbers of aliens into a growing country adds power to the population when they become assimilated without confusion. What we call "natural immunity" may find its analogue in some such process. But a common historical event has been a mass invasion of one people by another where the issue is not fusion, but conquest of one by the other. In such a case the national spirit of the invaded people is aroused to mobilize their mechanisms of defense, and these may or may not be adequate for victory.

But there is no likeness whatever between the national spirit and the military operations which it called into being. They have different rôles in the warfare. They may combine in a powerful union or, isolated, each may hurry to self-destruction.

The national spirit senses constantly the state of the country in its relations with alien peoples, always with a view of future possible conflict. Defense mechanisms of many kinds, each adjusted to deal with incipient trouble, are trained to go automatically into action. It may be plausibly maintained that this awareness of the country's interests, which is an attitude of the national spirit, finds an analogue in what is termed "allergy" in the phenomena of animal reinfections.

Allergy is the excitement-energy which precipitates the activities of prepared mechanisms of various kinds, here with violent inflammation and high fever, there with the gentle solution and disintegration of foreign germs and poisons without trace. Its whole import and reason for existence is protection of the organism—immunity. The more perfect and powerful that immunity, the less noise of conflict, the less evidence of allergy. But when the strongholds of the defenders are carried by assault—then allergic agony is futile to protect.

These analogues do not in themselves form admissible evidence, but they may broaden that field of suggestion from which the clews to most lines of productive research are picked.

No propaganda for the generation of public opinion can compare in speed and effectiveness with that universal hypersensitiveness to its own products which the growth of tubercle bacilli in a remote corner of the body has for the whole animal organism.

Physicists now soberly tell us that in the last analysis matter and energy are but different phases of the same something. Perhaps disputing biologists may come to see in vitalism and mechanism complementary views of the same subject.

As pain, fear and national spirit may be regarded teleologically as stimuli to conservative defensive reactions on the part of living units or communities, so perhaps we may properly consider allergy to represent a kindred agency which both generates hypersensitiveness to noxious inclusions and stimulates the body cells to form and operate defensive mechanisms against them. According to this view, in the language of social life, allergy is *in* immunity, but not *of* it.

HENRY SEWALL

UNIVERSITY OF COLORADO

LEAF DIAGNOSIS AND THE INTERPRETATION OF FERTILIZER REQUIREMENTS OF PLANTS

WITH an enthusiasm and tenacity that is reminiscent of Pasteur, H. Lagatu and L. Maume, of l'Ecole Nationale d'Agriculture de Montpellier, have made numerous investigations during the past ten years and published *in extenso* on this subject.[1] Recently some of this literature has been widely distributed among the agricultural experiment stations in the United States and elsewhere and has found partial recognition in this country.[2, 3]

The main principles enunciated by the authors are that when one of the major elements—nitrogen, phosphorus or potassium—is omitted from a fertilizer the other two will be absorbed proportionally more, thus leading to unbalanced nutrition, disturbed metabolism and reduced yield. These conclusions are contrary to the widely accepted and amply verified "Law of the Minimum" (Liebig) as applied to nutrition of plants. Experimental data from which

[1] *Compt. Rend. Acad. Sci.*, 179: 782, 932, 1924; 180: 1179, 1925; 182: 653, 1926; 184: 229, 1927; 188: 1062, 1929; 190: 389, 1137, 1516, 1930; 191: 579, 1930. *Compt. Rend. Acad. Agr.*, 13: 437, 548, 1927, and 14: 762, 1928. "Le diagnostic foliaire de la pomme de terre," *Ann. Sci. Agron.*, 47: 5: 596, 1930.

[2] SCIENCE, 70: 382, 1929.

[3] SCIENCE, 72: 425, 1930.

these deductions are drawn have been secured by the above authors, largely from the grape, *Vitis vinifera,* and more recently from the potato, *Solanum tuberosum,* the plants having been subjected to various régimes of nutrition. These records are primarily in the form of chemical analyses of leaves for N, K_2O, P_2O_5 and CaO.

In the extensive literature on plant nutrition, one finds frequent references to the general appearance of the plant, particularly its foliage, as being symptomatic of the deficiency of certain major or minor nutrient elements. In fact, Russell[4] gives a comprehensive list, which shows the apparent relationships between leaf appearance and lack of particular nutrient elements, but cautions that "if they (symptoms) are to be used in any but a general way, they should be studied by setting up the appropriate vegetative experiments" in order to verify the relationship between a particular symptom and its probable antecedent cause. Few[5] investigators, however, have gone so far as to base on chemical analysis of leaves either a diagnosis of the state of nutrition or to suggest or prescribe a program of soil fertilization. To the writers' knowledge leaf diagnosis (*diagnostic foliaire*), as conducted by Lagatu and Maume, is the first serious attempt in this direction. Is this method and technique really applicable in studies of nutrient requirements of plants, especially perennials? A partial confirmation comes from Wallace,[6] who found that the omission of potassium from a "complete fertilizer" applied to soil resulted in an increased absorption by *Ribes grossularia* of nitrogen and phosphorus. But when either nitrogen or phosphorus was omitted from the fertilizer then there was a decreased absorption of the other two elements.

The present writers had an excellent opportunity to test the principles proclaimed by Lagatu and Maume during the course of study of the physiology and metabolism of the apple (*Pyrus malus*). Dwarf trees were grown in pure quartz sand cultures and were supplied with nutrient solutions containing a constant and optimal concentration of phosphorus, nitrogen and potassium. For two groups of plants, however, the concentration of N and K in the nutrient solution varied respectively from 0 to 323 and 368 parts per million (ppm). At appropriate periods leaves and twigs were subjected to a chemical analysis for total N and K.

It is quite evident that the reduction or total omission of either nitrogen or potassium in no instance resulted in an increased potassium, respectively nitrogen content of leaves. Quite the contrary; with a decrease of nitrogen in the nutrient medium there was a decrease in potassium concentration in the leaves and with a decrease in potassium there was a decrease in nitrogen. Moreover, the relative concentration of N and K in twigs was exactly of the same order, but, of course, of a different magnitude. (More detailed data on the chemical composition of leaves and twigs of these trees will be published elsewhere.)

ANALYSIS OF LEAVES OF PYRUS MALUS

Nutrition		Chemical composition	
K and P	N, ppm	Total N, per cent.	Total K, per cent.
Constant	0	1.52	0.89
Constant	110	1.45	1.10
Constant	323	1.73	1.49
P and N	K		
Constant	0	1.38	0.94
Constant	126	1.45	1.10
Constant	368	1.50	3.04

These experiments furnish a concrete evidence and corroborate the negative results secured by Wallace and by Thomas[3] with the additional proof that, contrary to Wallace, the omission of potassium did not increase but evidently decreased the absorption of nitrogen. Consequently the principles proposed by Lagatu and Maume have not been verified, and Liebig's Law of the Minimum, as amplified by Mitscherlich[7] and others, must be considered as our best guide in the nutrition of plants.

But, disconsidering their codified generalization, why have Lagatu and Maume obtained seemingly consistent results with the grape? A part of this discrepancy between our and their results most probably is due to the fact that each species (in these instances *Vitis vs. Pyrus*) absorbs soil nutrients in a "physiologically balanced" proportion. Hence we have to deal here with selective or differential absorption, ionic antagonism and related phenomena (Loeb, Osterhout) as is suggested by Thomas.[3] Thus one is referred to the soil, more properly to the soil nutrient solution, and to the specific régime of fertilization of their plants as the primary causal source of Lagatu and Maume's results and Mitscherlich's[7] method of interpretation should be applied.

It seems to the writers that other equally, if not more, potent factors may be advanced to account for and to assist in interpretation of the data obtained by Lagatu and Maume. The concentration of certain soil nutrients in particular organs by no means re-

[4] E. J. Russell, "Soil Condition and Plant Growth," p. 102, 1927.

[5] Th. Remy-Brown, *Mittl. Deutsch. Landw. Gesell.*, 28: 416, 1913.

[6] T. Wallace, *Jour. Pomol. and Hort. Sci.*, 7: 1–2, 130, 1928.

[7] A. Mitscherlich, "Die Bestimmung des Düngerbedürfnisses des Bodens," 1925.

flects directly and definitely the rate or the proportionality of absorption of particular elements by a plant. Indirectly it may, and often does, indicate likewise the utilization of these elements by other organs, especially those of a higher metabolic rate and hence of a more rapid development. Due to a close physiological correlation of the various organs of a plant there is a continuous removal and diverting in unequal proportions of certain elements from particular organs, like leaves, and their reutilization for the development of other parts—fruits, shoots and roots. This is particularly true of woody perennials, like *Vitis*. It has been demonstrated by one of the writers[8] and by others[9, 10] that every one of the three elements—nitrogen, phosphorus and potassium —is removed from the lower leaves of the tomato plant whenever a shortage of a particular element for the vegetative extension or fruit development occurs. Yet Lagatu and Maume base all their interpretations solely on the analysis of basal leaves of fruiting canes of *Vitis* and of tuber-bearing plants of *Solanum*. It is not difficult to see how the concentration of any two of the three major elements of soil nutrients may increase in the leaves when one is *in minimo* for the development of other metabolically more active organs. This increase, therefore, is due to two major factors (not one): An unbalanced fertilization of the plant as a whole and an unbalanced nutrition of particular organs. In either instance (below and above ground, the "intake" and "outgo," absorption and utilization) the law of the minimum seems to hold true.

The detailed mechanism and interpretation of the metabolism and physiology of organic correlation may be analyzed on the basis of "metabolic gradients," as suggested by Child.[11]

A. E. MURNEEK
E. J. GILDEHAUS

UNIVERSITY OF MISSOURI

THE MECHANISM OF CROSSING-OVER

DARLINGTON recently published in SCIENCE[1] (73: 561–562) a criticism of my work[2] which might give the reader the impression that Darlington was not given credit for previous suggestions concerning the mechanism of crossing-over. In 1929 Darlington[3] did suggest that crossing-over is due to a reduction of chiasmata by breaking. In the three sentences which refer to this theory (pp. 50, 51 and 52) he does not explain how chromatids might break and recombine nor does he present any evidence to show that he had any conception of the significance of his suggestion. However, his suggestion was acknowledged (Sax, 1930, p. 209) as follows: "It seems very probable, however, that crossing-over between homologous chromatids is associated with the reduction in the number of chiasmas between diplotene and diakinesis, as Darlington (1929) has suggested."

In a paper which was published shortly before mine went to press, Darlington[4] does not mention his earlier suggestion that crossing-over is caused by breaks in the chiasmata, but expresses the view that cross-overs determine chiasma formation. The cytological evidence for this view is supported by a few diagrams and text figures which by no means can be considered as a "cytological demonstration of genetic crossing-over."

The fact that Darlington discarded his earlier "suggestion" on crossing-over in no way discredits or weakens my theory. Darlington has made so many assumptions concerning chromosome pairing, and has changed his mind so frequently that one would necessarily have to cite one of his numerous theories in any discussion of crossing-over.

Darlington also states that I have used diagrams and terminology borrowed from his 1929 paper. The only term used which might be credited to Darlington is "terminalisation." No figures were borrowed from him, although my figures 9 and 10 are based on his work. For this phase of my interpretation of crossing-over Darlington is given credit as follows: "The behavior of the chromosomes in triploid Hyacinths described by Darlington (1929) seems to offer an explanation of triploid crossing-over" (p. 214). The fact that Darlington did not fully appreciate the genetic significance of his cytological results in no way discredits his ability as a technician.

According to Darlington, my "genetical remarks might be taken to favor either hypothesis—for there is no decisive evidence between them." This statement is of more than doubtful validity. In none of Darlington's papers is there any explanation of the cause of breaks in chromatids, why they unite in new associations, why the two homologous chromatids almost always cross over at the same loci, of how gene duplication or deficiency could occur, or why one cross-over interferes with another. In his most re-

[8] A. E. Murneek, *Plant Physiol.*, 1: 3–56, 1926.
[9] J. H. MacGillivray, *Jour. Agr. Res.*, 34: 2: 97, 1927.
[10] G. Janssen and R. P. Bartholomew, *Jour. Agr. Res.*, 38: 8: 447, 1929.
[11] C. M. Child, "Individuality in Organisms" and "Senescence and Rejuvenescence," Univ. of Chicago Press, 1915. Also *Plant Physiol.*, 1: 1–3, 1926.
[1] C. D. Darlington, "The Mechanism of Crossing-over," SCIENCE, 73: 561–562, 1931.
[2] Karl Sax, "Chromosome Structure and the Mechanism of Crossing-over," *Jour. Arnold Arb.*, 11: 193–220, 1930.
[3] C. D. Darlington, "Meiosis in Polyploids. II. Aneuploid Hyacinths," *Jour. Gen.*, 21– 17–56, 1929.
[4] C. D. Darlington, "A Cytological Demonstration of 'Genetic' Crossing-over," *Proc. Roy. Soc.*, B. 107: 50–59, 1930.

cent paper Darlington[5] (1931) simply ignores these difficulties and has to assume several improbable hypotheses to account for the absence of crossing-over in the *Drosophila* male. All these genetic phenomena are easily and logically explained on the theory that crossing-over is due to breaks in chiasmata (Sax, 1929). The genetic analysis of chromosome behavior in *Drosophila* as developed by Morgan and his associates is a far more precise and accurate tool for the analysis of chromosome behavior than any method now available to the cytologist. Any theory of crossing-over which does not meet the strict genetic requirements can not be considered seriously.

Darlington also states that my "genetical remarks" . . . "are vitiated as evidence by his using the word chromosome in three different senses." In the paragraph referred to the term chromosome was not confined to one particular phase of meiosis, as should have been the case, but by no consistent or logical method of reasoning would it be possible to conclude that the word chromosome was used in three different senses or that the genetic evidence is vitiated.

KARL SAX

ARNOLD ARBORETUM,
HARVARD UNIVERSITY

THE DIVINING ROD AND FAKERS

I READ with much interest articles in SCIENCE as to belief in the efficacy of a divining rod. My explorations have taken me throughout the West and the South for nearly forty years. We have frequently tried experiments, or rather permitted those who have faith in "rods," to test for buried objects. I have never, in all my experience, known a single instance in which the test was of either practical or scientific value.

To be specific: During the exploration of the Cahokia Mounds, southern Illinois, there was much publicity, hence various persons with divining rods appeared. One man's apparatus was different from the usual forked stick. It consisted of a sphere about the size of a baseball at the end of a leather thong. This contained certain ingredients which were "secret" and he claimed the ball would oscillate over treasure as well as Indian remains. We afforded him facilities —the test was a failure. A rather wild-eyed individual appeared and told me that if I would give him a "thigh bone of a big Injun" he could lay hands on said bone and thus come in contact with the Indian's spirit. He was assigned quarters in a tent for a few hours, left in seclusion, and loaned a femur. I asked him afterwards how he, speaking English, could communicate with a prehistoric Cahokian. His reply was that all people in the spirit world spoke the same language!

The next season a middle-aged woman appeared and claimed ability to commune with departed Indians "now residing in the spirit world." My assistant and a workman were inclined to make sport, yet we gave her a tent and table at her request. She communed for two hours. Then she announced that we would make a great discovery somewhere within 2,000 feet, but was not specific. The price to locate accurately was ten dollars!

Some weeks later a man appeared who claimed that he was an astrologer, contended that he had spent numbers of moonlight nights wandering over the Cahokia Mounds, that said mounds were orientated according to certain stars, etc., that if we would employ him he would spend clear nights, when stars were visible, in his studies. He entered into a lengthy and somewhat erratic explanation, the upshot of which was that there were certain points where lines drawn from one star to another crossed, and the point exactly beneath [on earth] would yield important archeological specimens, etc. He was very insistent but could not designate any precise spot in which we should excavate. He also desired pay.

In southern Ohio a "Hill Billy" appeared in our camp with the usual divining rod, and was given a thorough test. We sent to the bank and got $25 in silver. We buried it secretly a few inches below the surface, scattering dead leaves, removing all fresh earth. The ball would sway at various points but didn't move when our friend passed over the "buried treasure."

At Cartersville, Georgia, a man came to our camp with his "divining rod" talk and begged for a chance to exhibit his prowess. He remained with our party two days and was given two or three men with shovels to excavate wherever he indicated. It seems that the other men made sport of their comrades who accompanied "Mr. Divining Rod" and, therefore, to keep peace in my field party it was necessary to detail a fresh crew each half day! Many test pits were sunk, nothing was found, but the owner always had the same excuse of any other person with a divining rod, *i.e.*, that the treasure was further down. In brief, each one had a good alibi. In instances where we struck ledge or boulder clay, or outcrop of limestone, etc., the explanation was the same.

In every instance, in several states, I kindly explained to these people afterwards the utter futility of such efforts. Each one, however, had supreme faith in his divining rod. Our friend in Georgia, who was with us longest, a simple-hearted and ignorant man whose family, I was informed, was in need, listened with some impatience when I urged that he throw away his rod and go to work. He answered

[5] C. D. Darlington, "Meiosis in Diploid and Tetraploid *Primula sinensis*," *Jour. Gen.*, 24: 65–96, 1931.

in words which would apply to all these people, "Mister, I might give up my house, or even the old woman, but I ain't going to give up the divining rod. Some day it will make me rich!"

WARREN K. MOOREHEAD

PHILLIPS ACADEMY,
ANDOVER, MASS.

THE VISIT OF DR. JAKOB E. LANGE

DR. JAKOB E. LANGE, well-known Danish student of the mushrooms, will arrive in New York the middle of August for several weeks of collecting in the northeastern United States and eastern Canada. He wishes to study especially the parallelism and identity of American and European species of Agaricaceae. A definite itinerary has been arranged. Inquiries regarding its details may be directed to Dr. C. W. Dodge at Pawlet, Vermont.

From August 28 to September 2 inclusive Dr. Lange will be at Ithaca, New York. The region about Ithaca is especially interesting to him because Atkinson published over a period of years on locally collected materials. Fungus forays will be made daily to near-by points of interest in the effort to see a large number of species.

In order that the conceptions of species as held by Peck, Atkinson, Kauffman and other older American workers in the group may be clearly understood, it is imperative that Dr. Lange be enabled to exchange ideas in the field with their students. To this end American mycologists, especially those interested in mushrooms, are urged to come to Ithaca and cooperate in making these forays a success. Students with only a minor interest in the Agaricaceae will also be welcomed, and the forays will be arranged in such a manner that collecting in other groups will be fruitful. Incidentally, the Atkinson herbarium has been put in good order in recent years, and is now available for consultation in the new Plant Science Building at Cornell University.

Those who plan to attend the Ithaca forays are asked to notify the undersigned at as early a date as possible. Arrangements will be made for lodging, meals and transportation at reasonable rates. Information concerning these items, or other features of the plans for the forays will be gladly given.

H. M. FITZPATRICK

CORNELL UNIVERSITY,
ITHACA, N. Y.

REPORTS

UNITED STATES GEOLOGICAL SURVEY UNPUBLISHED LITHOGRAPHIC PLATES OF VERTEBRATE FOSSILS FOR DISTRIBUTION

Two hundred and thirteen of the lithographic plates prepared under the direction of Professor Othniel C. Marsh are now being assembled to distribute for research and educational purposes to the principal active centers and libraries of geology, paleontology and comparative anatomy in this country and abroad. The distribution is by permission of the director of the United States Geological Survey through the agency of the Department of Vertebrate Paleontology in the American Museum of Natural History. There are also the original Cope lithographic plates, some from "The Vertebrata of the Tertiary Formations of the West," some from the Cope-Matthew volume, some which have never been published.

Seventy-one sets of these assembled plates have already been sent out to various institutions in this country, seventy have been prepared for distribution abroad and three hundred and fifty are to be kept in reserve. Applications by libraries and laboratories for plates from this reserve supply should be addressed to the Curator of the Department of Vertebrate Paleontology of the American Museum.

In this connection it seems of interest and importance to review briefly the circumstances surrounding the long history of these lithographic plates which began probably as early as 1878 when Marsh was working for the King Survey and just prior to his appointment as vertebrate paleontologist of the United States Geological Survey, formed in 1879. The Marsh-Cope plates drawn on stone by Mr. F. E. Berger under the direction of Professor Marsh are masterpieces of the art of lithography which had reached a culminating point in England, Germany, France and America during the great foundation period of Leidy, Cope and Marsh, between the year 1850 when Leidy began his work and 1899 when Marsh's work was terminated by his death. No modern reproductions can compare with the beauty of Berger's original plates, so that this opportunity of securing the originals will, it is expected, be eagerly availed of, and prompt response will be given to any application.

The enumeration of the plates available for distribution and research is as follows:

The Stegosauria, 63 plates
The Brontotheriidae, 60 plates
The Sauropoda, 90 plates

On the death of Professor Marsh in 1899, Director

Walcott, of the United States Geological Survey, invited the writer to supervise the preparation of Marsh's four incomplete monographs. The materials were found to consist of over 200 carefully prepared lithographic plates, of drawings and wood engravings, some bibliographies and about one hundred pages of rough pencil notes and memoranda. There was no manuscript; the entire text of the four monographs remained to be written. It was obviously appropriate to assign the Ceratopsia Monograph to John Bell Hatcher, because the discovery and collection of these animals was the greatest single achievement of his remarkable life; he had devoted four arduous years to bringing together these magnificent horned dinosaurs for Yale University and the National Museum. Hatcher entered upon this research in July, 1902, with his usual ardor and thoroughness; on July 3, 1904, when he was stricken down the work was taken up and completed in a most admirable manner by Professor Richard S. Lull, of Yale University; it was published in 1907 as United States Geological Survey Monograph 49.

The next volume, the Stegosauria, was assigned to Mr. Charles W. Gilmore, of the United States National Museum, who began work in 1906 and, although the monograph as originally projected was not finished, he published in 1914, as United States National Museum Bulletin 89, the work entitled "Osteology of the Armored Dinosauria in the United States National Museum, with Special Reference to the Genus Stegosaurus," which covers only the material in the United States National Museum, but which established its author as a leading authority on these armor-plated dinosaurs.

Under the name "The Titanotheres of Western North America," research on the Brontotheriidae was begun by the present writer in the year 1900 and with the masterly aid of William K. Gregory was completed and published as United States Geological Survey Monograph 55 under the full title of "The Titanotheres of Ancient Wyoming, Dakota and Nebraska" in the year 1929.

Meanwhile research on the remaining monograph, the Sauropoda, was independently begun by the present writer about 1902 with Mrs. J. K. Mosenthal and, in 1912, with the aid of Dr. Charles C. Mook, of the American Museum staff. The title of this volume, if the plan for its issue can be carried out, will probably be "The Sauropoda of the World."

The problem of preparing "The Sauropoda of the World" differs radically from the problems involved in "The Titanotheres of Ancient Wyoming, Dakota and Nebraska": first, in the fact that we have little or no antecedent history of this remarkable group. The Sauropoda suddenly flash into being, to our present knowledge, towards the close of Jurassic time, fully formed and widely differentiated into a number of very distinct types, all of gigantic size and well fitted by their long limbs for the world-wide migrations which carried them to every continent, even including Australia. The central problem in the Sauropoda Monograph will, therefore, be the distinction of five or six outstanding generic or sub-family types together with the more or less speculative problem of their origin and the intensely interesting problem of the causes and means of their world-wide distribution and finally their extraordinary explosive extinction. Remarkable additions to our knowledge have been made since the superb Marsh lithographic plates, and very clear diagnoses of Sauropod characters were given by Marsh. Valuable collections have been made principally by the Carnegie Museum of Pittsburgh, by Wortman, Hatcher and Peterson under the direction of Dr. W. J. Holland.

The discovery in East Africa of the magnificent Tendaguru deposits explored by Eberhard Fraas, of Stuttgart, and others have been given preliminary descriptions by Dr. Werner Janensch, Dr. Hans Reck and Dr. J. G. Pompeckj, of Berlin. Dr. F. von Huene, of Tubingen, has recently revised Richard Lydekker's monographic work on the Sauropods of South America. Of great significance is the discovery of scattered remains in the Desert of Gobi by the Central Asiatic Expedition under Andrews and Granger, revealing what may have been the central or ancestral region in which these great animals enjoyed their original evolution. It is planned, now that the Titanothere Monograph has been completed, to renew the researches on the Sauropoda with the attempt of coordinating this great mass of new material with the original ground material made in the discoveries and writings of Marsh and of Cope.

HENRY FAIRFIELD OSBORN

SCIENTIFIC APPARATUS AND LABORATORY METHODS

A SIMPLE ULTRA-CENTRIFUGE

IT is hardly necessary to emphasize the value of the centrifuge to science in general. Its numerous uses in so many fields of experimental investigation have made it almost a necessary laboratory tool. As a consequence of this wide usage considerable energy has been directed toward the development of centrifuges with our modern high speed machines as a result. However, there is still very much to be desired in the way of improvement. Many problems of ut-

most importance remain unsolved because of the insufficient separating power of our best centrifuges. Among the factors which limit the separating power of the centrifuge are, of course, rotational speed, suitable bearings, strength of materials, troublesome vibrations, and simple means of making the centrifuging continuous. During the last few years we have been engaged in a series of researches that required apparatus for obtaining high rotational speeds. Some time ago we undertook to apply this comparatively simple technique to the centrifuging problem and have obtained such remarkable results that it seems worth while to call the attention of others to its possibilities.

The method of whirling the centrifuge consists in both driving and supporting the rotor by means of a whirling cushion of air. By this procedure the maximum rotational speed is apparently limited only by the molecular speed of air and the strength of the rotating parts. In some of our experiments we have obtained rotational speeds of approximately one half million revolutions per minute and centrifugal forces over a million times that of gravity. The experimental arrangement for obtaining these high rotational speeds is a modification of one used by Henriot and Huguenard (*Comptes Rendus*, 180: 1389, 1925; *Jour. de Phys. et Rad.* 8: 443, 1927). A detailed description of the method of rotation and the ways of measuring the rotational speed have been given previously (Beams, *Review of Scientific Instruments*, 1, 667, 1930) so that only a very brief sketch of this part of the apparatus need be given here.

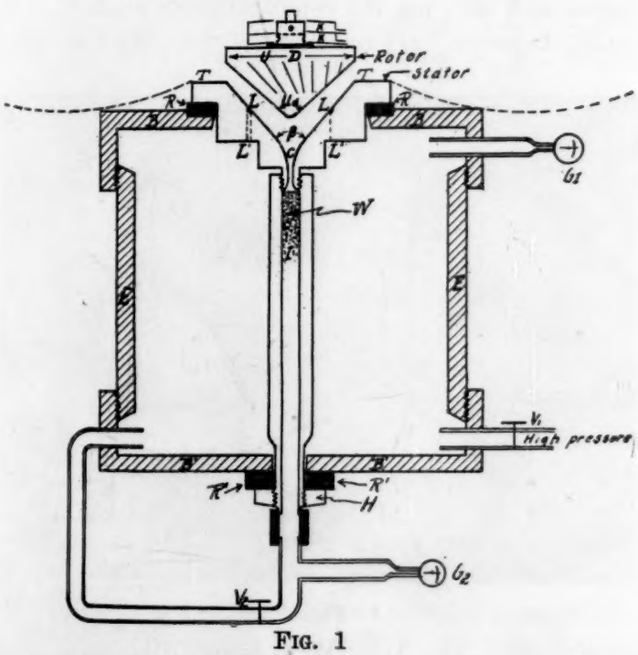


FIG. 1

Referring to Fig. 1, air from a compressor is admitted to the chamber through the valve V_1, until the pressure gauge G_1 registers the desired pressure. V_2 is also opened until G_2 reads the proper pressure, which is found by trial. The rotor is then placed in the position shown in the drawing. The air jets from LL′ and C impinge upon the flutings of the rotor, raise it, and start it rotating. Immediately it seeks a position of stable equilibrium a fraction of a millimeter above the surface of the stator, where it continues to rotate. The air entering through C stabilizes the motion and makes it possible to obtain a greater range of speeds as well as gives a means of easily adjusting for different weights and slightly different shapes of the rotor. In the case of the rotor, or centrifuge proper, the problem immediately arises of how to get the material to be centrifuged in and out without touching or rubbing while the rotor is at full speed. Fortunately this problem can be solved in several ways. If a small amount of material is to be centrifuged the arrangement shown in Fig. 2 is satisfactory. The rotor is filled with the

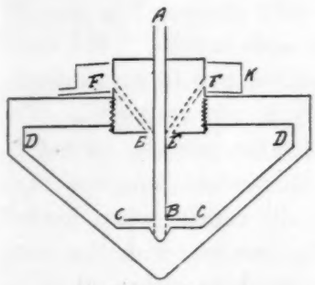


FIG. 2

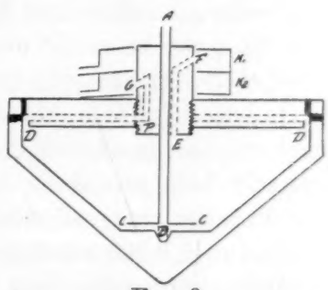


FIG. 3

liquid and allowed to come to full speed. After the components of the liquid have had time to separate, a somewhat heavier substance, with which the liquid under investigation will not mix, is very slowly dropped into the tube AB (which is slightly larger at B than at A). This forces the lighter fractions out of the tubes EF where it is collected by the cylindrical collector K. Fig. 3 shows a scheme where the centrifuging is continuous. The rotor is first filled through the tube AB then brought to full speed and allowed to rotate for some time. More of the liquid to be centrifuged is then slowly introduced through AB and thus emerges at C. Its heavier constituents move to D and the lighter to E where it is forced up the small hole EF and collected by the collector K_1. The heavier material is forced out through the tube DPG and collected by K_2. There are twice as many tubes as shown in the drawing. MP has a symmetrically placed identical tube while NE (drawn in Fig. 3 at right angles to its real position) also has a symmetrically placed tube to keep the centrifuge in balance. The distance of the tubes PG from the axis of rotation and the height of its opening depend upon the approximate ratio of the densities of the substance to be separated, so it is necessary to design the rotor specially for each ratio

of densities. It will be recalled that the practical separating power of a centrifuge depends not only upon the maximum magnitude of the rotational speed but upon the radius of the rotor as well. It is found desirable to make the peripheral velocity as large as possible. Fig. 4 shows an arrangement that

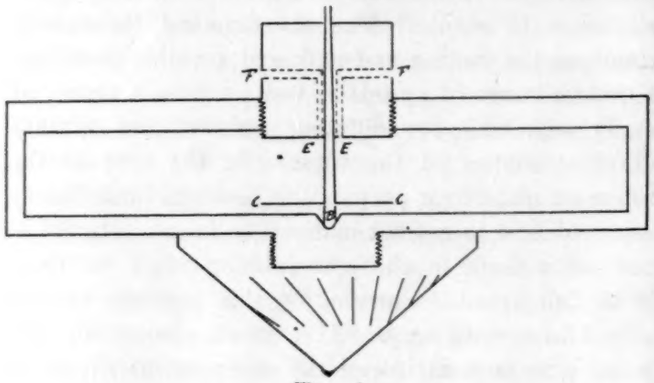


FIG. 4

has some advantage and is very stable. The size of the rotor can be varied over wide ranges. We have used them, for example, from 1 cm to 10 cm in diameter, but the size apparently is not restricted. Peripheral speeds of over 3×10^4 cm per sec. have frequently been attained. A convenient size one inch in diameter made of steel will rotate, when loaded with water, 3,500 revolutions per sec. with the compressor giving only about 1,200 cubic inches of air at a pressure of 100 lbs per square inch above one atmosphere, per minute. For this the angle of the stator $\beta = 91.5°$ and the angle of the rotor $\alpha = 103°$. The 8 holes LL′ were drilled with no. 73 drill.

By adjusting the air pressure the speed of revolution can be varied over wide ranges. The speed, however, remains remarkably constant when the pressure is held constant. Another striking thing is the absence of vibrations in the rotor when filled with a liquid and the consequent reduction of stirring or remixing to a minimum. The theory of separation by centrifuging should, therefore, hold with good approximation.

J. W. BEAMS
A. J. WEED

ROUSS PHYSICAL LABORATORY,
UNIVERSITY OF VIRGINIA

A METHOD FOR COMPARING GROWTH RATES BY MEANS OF A PROTRACTOR

IN growth studies on plants and animals, the investigator, in his examination of the data, wishes to compare not only the growth increments, but also the *rates* of growth. The former may be done by the usual graphs, plotted directly from his records, but when he is contending with large amounts of data the numerous calculations of growth rates are time-consuming and become tedious. The writer has employed a simple and rapid method for the inspection of growth rates directly from the increments graphs. Although the method is not exceedingly accurate, it has proven to be of considerable aid in the general study of growth data.

The procedure is as follows: The customary growth increment graphs are plotted upon standard coordinate paper, being careful to locate each point accurately and sharply, and, in any series to be compared, the distances along the abscissa and ordinate allotted to the units of time and growth must remain constant throughout all sheets of graphs.

The rate of growth is the *slope* of the graph between any two points or observations. The value of this slope in degrees is easily determined by means of an ordinary transparent protractor. One has simply to place the point of origin of the protractor exactly upon one point along the graph, and then to rotate the protractor until its basal axis is directly over the point marking the next observation. The value of the slope between these points is read from the protractor over a line which is the continuation of the ordinate of the first point selected. This value, for convenience, is written beside the particular interval of the graph measured. Unfortunately, for this work, protractors are so numbered that the "90°" mark is at the top and the "0°s" are to the right and left at the bottom, with the result that, the more rapid the growth rate, the smaller is the indicated angle corresponding to the slope. To rationalize this, one may subtract from 90 degrees the value found, and then use the complimentary angle. It is better, however, to renumber the protractor so that

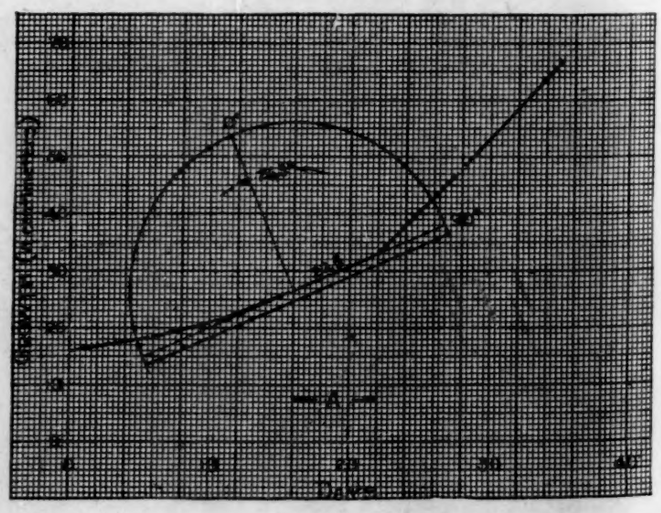


FIG. 1

"0°" occurs at the top, and "90°" at the bottom to the right (the first quadrant only is used). Fig. 1 shows a growth increment graph with protractor in position for measuring the slope of interval "A."

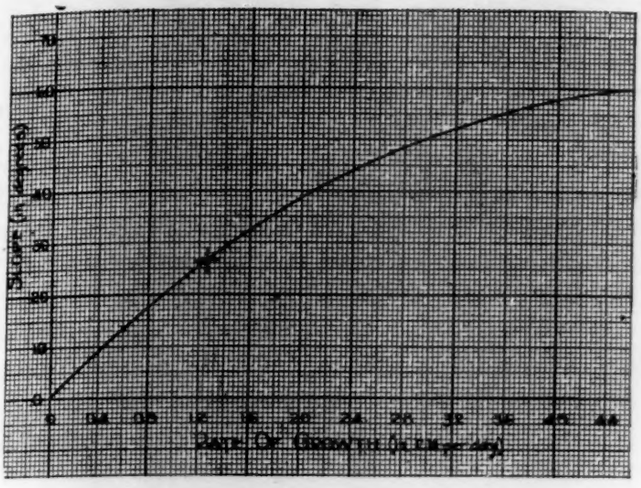


FIG. 2

The values found are arbitrary in any series of graphs, but they are suitable for comparison either by direct inspection or by using them for plotting other graphs showing rates of growth. If it is desired, however, the slope may be translated quickly to absolute rate of growth through reference to a curve, which is obtained by calculating from the growth data the actual rates corresponding to a few representative slopes. Such a curve of reference is shown in Fig. 2. When the slope of 26.5°, as found for interval "A" in Fig. 1, is referred to the curve in Fig. 2, it will be seen that this slope corresponds to a growth rate of 1.26 centimeters per day.

The protractor method for determining growth rates is very simple and rapid, and it should be particularly useful when large quantities of growth records have to be studied.

E. M. HARVEY

OREGON AGRICULTURAL EXPERIMENT STATION

SPECIAL ARTICLES

ON MOLECULAR ORGANIZATION IN AMEBAN PROTOPLASM

THE question whether a submicroscopic organization exists in protoplasm has been considered with increasing frequency during the past several decades. Among the more recent discussions come to mind those of H. Przibram, R. G. Harrison, L. J. Henderson, J. Needham, E. J. Lund, C. V. Taylor, G. H. Parker, W. T. Bovie and others. The wide-spread and persistent interest in this subject is presumably due to the belief that the proof of the existence of submicroscopic organization would not merely add a new series of facts to science concerning the nature of protoplasm but would, in addition, serve to bring many observations on the form and behavior of organisms into relation which now stand by themselves. And as a consequence, it would also serve as a good tool for further investigation in a number of lines of work.

The data presented herewith seem to me to constitute evidence in support of the hypothesis of molecular organization in the protoplasm of four species of amebas. Other data will also be presented as presumptive evidence tending to show that similar organization may be general among organisms and that certain large groups of hitherto unrelated observations fall into a coherent system under this generalized hypothesis. Since the entire argument is based ultimately on molecular movements and displacements, a statistical treatment is probably the most appropriate method of handling the data.

I. When an ameba is placed on a thin glass rod or within a fine capillary tube of suitable size, the path which the ameba makes on the rod or within the tube is a helical spiral of greater or less regularity, depending somewhat upon the individual, the species and other factors. The direction of the path may be to the right or to the left, for varying lengths of time, and may alternate frequently during the course of a few hours. Of the four species investigated, three are predominantly *left,* making each about 1.4 left turns to 1 right; the other species, which readily hypertrophies, is predominantly *right* to a slightly greater degree.

Now if one takes a convenient, small unit of measurement, such as one half turn around the rod or tube, and then measures all the right and left sections of path to the nearest half turn and arranges the sections in a frequency series based on length, a characteristic distribution is obtained for each species. Thus for the 4 species: *Rugipes bilzi, Mayorella conipes, Trichamoeba schaefferi, T. sphaerarum,* the distribution in the higher categories closely approximates the exponential series represented, respectively, by the formulas:

$$2^{-\left(\frac{y-1}{2}\right)}, \; 2^{-(y-\frac{1}{2})}, \; 3^{-(y-\frac{1}{2})}, \; 4^{-(y-\frac{1}{2})},$$

where y is the length of any section. The data, representing 316, 3250, 322 and 615 sections respectively, of the above named amebas, are shown graphically in the figure. (The points representing the lower categories on the figure, which have drifted off the theoretical curve and are believed to represent the operation of an added factor, will be discussed in a more extended paper to be published soon.) For convenience of graphic representation the observed number of sections are proportionated so that the highest category, y^o, equals 1,000 and the log of the

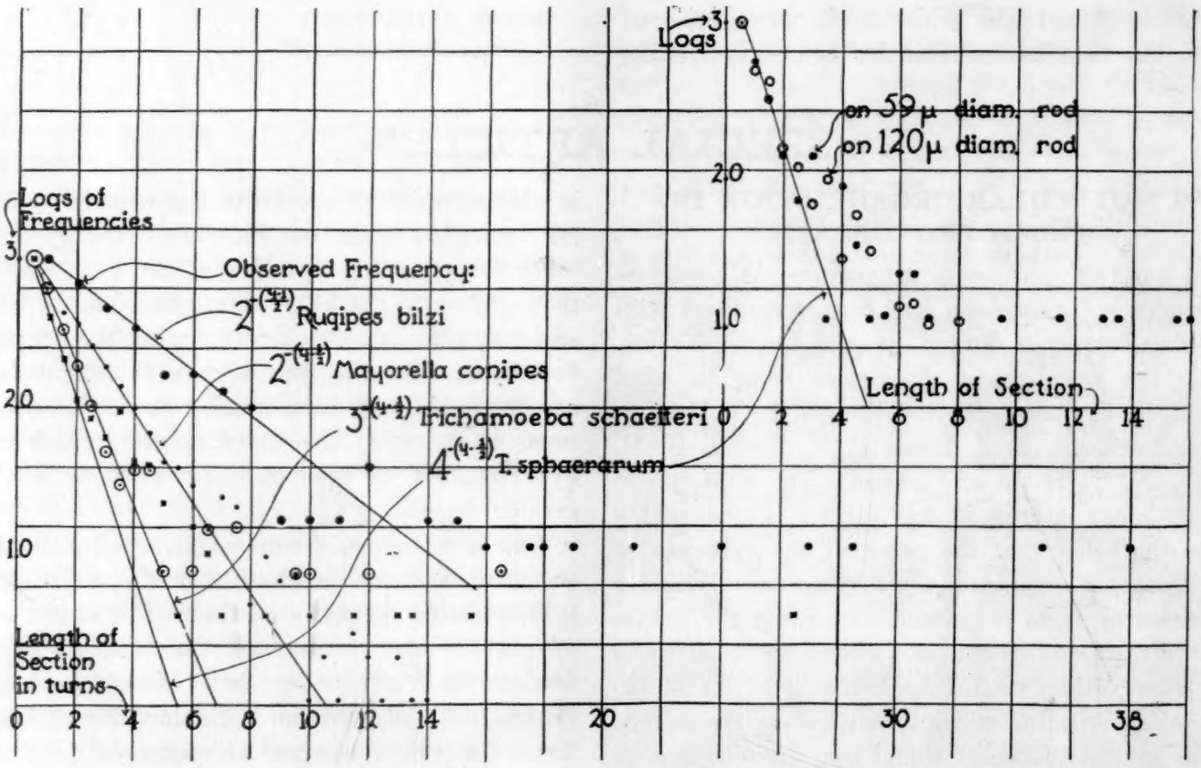
numbers of sections in each category is plotted against the length of the section.

(1) It will be noticed that the first two series noted above are alike numerically but that in the first ameba the steps or nodes in the path where changes of direction are most likely to occur, are twice as far apart as in the second, third and fourth amebas.

(2) The series differ by *integral* steps.

(3) A particular frequency may occur independently of the particular length of the internode between changes of direction.

In general, these data show that there is a succession of "weak points" or nodes somewhere in the neighborhood of three and one half body-lengths of the ameba apart, for the last 3 amebas, and 7 body-lengths for the first ameba. It is at these weak points or nodes that the direction of movement is more likely to change than at any other points. Statistically, the periodicity is remarkably regular.



That the periodicity is not due to the recurring asymmetry of light and geotropic influences as the ameba moves around the rod, is shown in the graph in the upper right corner of the figure. Two rods of different diameter were used in this series of observations, one 59 microns and the other 120 microns in diameter.

(1) The frequency formula, each based on one half turn of the rod as a unit of measurement, is exactly the same for both rods, in the higher categories. This would be expected mathematically.

(2) For equal amounts of space covered on the 2 rods, the number of sections is not the same on both, nor about twice as great on the thin rod, as might perhaps be expected, but there are actually 40 per cent. more sections on the thin rod.

(3) The experimental conditions having been the same in both cases, the results stated in the paragraph above can not have been due to asymmetries in lighting or gravity but must clearly involve some function of the curvature of the rod.

(4) Whatever the length of the node as produced experimentally, the frequency of the breaks (degree of cohesion or field strength) remains the same.

The frequency of changes of direction or field strength of the molecules of the first 2 species of amebas mentioned above is therefore exactly the same, but the nodes are twice as far apart in the first as in the second ameba. But in the second, third and fourth amebas the nodes are the same distance apart but the field strength of the molecules differs systematically.

II. Another set of experiments bearing on molecular organization is concerned with the effect of light. Two of the above named amebas, *conipes* and *sphaerarum,* were subjected to the same technique as described above, excepting that the light was alternated in some experiments every 30 minutes, in others every 45 minutes, in others still, every 60 minutes, with light of lower or higher intensity. The intensity ratios, varying from 2 to 8, were secured by use of a light wedge, sector wheel or distance difference.

For the purpose of this discussion amebas may be divided into 2 classes: a larger class in which the total number of left turns is greater than the number of right turns; and a smaller class in which the right turns similarly predominate. It has been found experimentally that amebas predominating in left turns react to changes of light intensity that is exactly opposite and approximately equal to the reactions of predominantly right amebas.

(1) In both types of amebas belonging to the species *conipes,* the average change of the left-right ratio of spiral turns in 5 different pairs of intensity alternations, comprising 2,000 turns, was found to be 18.6 per cent. more right turns in the *higher* intensity for the *left* amebas; and 18.4 per cent. more right turns in the *lower* intensity for the *right* amebas.

(2) In *sphaerarum,* using 3 pairs of intensity alternations and comprising 1,009 turns, 22.4 per cent. more right turns were produced in the *higher* intensity by predominantly *left* amebas, while 24.1 per cent. more right turns were produced in the *lower* intensity by the *right* amebas.

(3) In the light of physical science these results, obtained by varying the light intensity, are interpreted as due to the action of light-activated atoms in the molecules, which causes these molecules to change their spiral twist from left to right or *vice versa,* depending upon the conditions of the experiment. But if these light effects are considered in connection with the periodicities and internodes, all of which are related integrally, then only one conclusion seems possible with respect to the nature of the underlying mechanism: it must be particulate and therefore molecular. This means, further, that high correlation between a structural or functional characteristic of an organism and a specific left-right ratio of spiral turns is a sign of a corresponding stereorelation between the atoms in the spiral molecule. In other words, high correlation between a specific left-right ratio and the presence of any characteristic of an organism means that the characteristic is a steric property of the spiral molecule. (The converse is of course not true, since there must be many steric displacements possible which do not throw the molecule as a whole from a left spiral twist into a right, or *vice versa.*)

Seeing that the spiral molecule is labile enough to be easily changed from one direction of twist to another by merely changing the light intensity one would naturally expect other physical or chemical agencies to produce similar changes. In illustration of such expectation two correlations, numbered III and IV are given immediately below. Following these are two other correlations, numbered V and VI, which point to a probable and very interesting statistical relationship between two very wide-spread characteristics of organisms and their distinctively steric characteristics.

III. Two of the species of amebas referred to above, *bilzi* and *schaefferi,* frequently hypertrophy markedly under some cultural conditions, when they become strongly right-turning and reproduction is rare or absent.

IV. A normally left-turning *sphaerarum* becomes strongly right-turning within 30 minutes after eating a large and easily digestible food object, and does not become predominantly left-turning again until after several hours have elapsed.

V. The direction of spiral twist of body in animals is negatively correlated, statistically, with the direction of spiral movement; in plants the correlation is apparently positive.

All motile organisms move spirally when guiding senses are not functioning, so far as known. By experiment and observation this has been found to hold true from bacteria to blindfolded aviators. Many organisms have spirally twisted bodies. Many others have some prominent unpaired organ spirally twisted. Many others, again, show no conspicuous spirality. The lack of spirality in the body of an organism is, however, not correlated with its property of moving spirally; for this class of organisms moves as definitely spirally and to the same degree as does the other class. The *mechanism of spiral movement* is, therefore, independent in its presence and in its function of the gross or visible spiral structure of the body. But there is a statistical correlation between the *direction* of the visible spiral structure and the *direction* of spiral movement.

(1) Of the 168 species of ciliate protozoa (Bullington), 146 belong to right-spiral structure groups and 22 to left-spiral structure groups. Of the 146 species belonging to right-spiral structure groups, 104 swim to the left spirally and 42 swim to the right. Of the 22 in the left-spiral structure group, 20 swim to the right and 2 swim to the left.

(2) Of 40 species of flagellates studied quantitatively, belonging mostly to the Euglenoidinea, almost if not quite all *show* left-spiral structure and all were found to swim to the right.

As stated above, many organisms swim spirally in which no spiral structure has yet been found; but if one uses the data derived from the study of the 208 species of ciliates and flagellates, which show definite negative correlation between direction of spiral structure and spiral movement, then the 102 species of rotifers studied, of which 92 swim to the right and 10 to the left, may be said to constitute a left-spiral structure group. Likewise also the 22 species of tunicates and salpas, which have been found to swim to the right, may be said to constitute a left-spiral structure group.

In addition to these data may be mentioned the snails, of which 15,230 are right spiral in structure and about 730 left (Leunis), but no observations have been found recorded as to direction of spiral movement of larvae or adult of any snails. Three hundred species of dinoflagellates are structurally left spiral but the movements of only about a dozen have been studied carefully and of these, half are right-turning, one left, and the rest alternate from right to left, but the predominance of the last group is not recorded. Volvox and some of its congeners are known to swim to the left, as do also the larvae of about 40 species of worms, bivalves, echinoderms and coelenterates—all that have been studied.

Summarizing, it is seen that groups of organisms with left-spiral structure (assumed in rotifers) swim to the right predominantly in the ratio of about 10 right to 1 left, or greater; while right-spiral structure groups swim left predominantly in the ratio of about 2.3 left to 1 right.

In plants, however, the situation is different. The cyanophyceae and spirochaets are twisted to the left and the motile species swim to the left. The spermatozooids of the characeae, mosses and ferns, so far as recorded, swim in the same direction as the body is twisted. Among the spermatophytes, 48 out of 65 climbing plants turn to the right (Darwin, Cook). From the few observations recorded, motile plants are evidently unlike animals in that the plants move in the same direction as their bodies are twisted, but they agree with animals in that the lowest groups are almost all left in structure and the highest predominantly right in structure.

VI. The incidence of sex among the different groups of animals is also statistically correlated to a greater or less degree with right-spiral structure, and apparently also in plants. Taking first the left-spiral groups among animals, we find, with only a very few exceptions, asexual reproduction among the flagellates; very great reduction in the size of the male among the Vorticellidae and the rotifers, and hermaphroditism among the tunicates. The large groups closely allied to these, respectively, are right spiral in structure and sexual equality is the rule. Of the living gastropods about 43 per cent. of the species belong to the (hermaphroditic) Euthyneura and 57 per cent. to the (unisexual) Streptoneura (Leunis). Approximately 10.4 per cent. of the Euthyneura are twisted in a left spiral, while less than .2 of 1 per cent. of the Streptoneura are left in structure. Observations on about 40 species of worms, echinoderms, bivalves, crustacea and vertebrates, which constitute the remainder of definite data on this subject, indicate that these groups are predominantly right spiral in structure, since they *move* spirally to the *left;* the data are, however, too few in this particular case to do much more than excite a lively interest in collecting more data.

Among the plants the same condition apparently holds. The observations are few and difficult to make, requiring first the development of a new technique for the ready detection of spirality. The lower plants; spirochaets, cyanophyceae, spirillum, are left in structure and sexless. The spermatophytes, as judged by the climbing plants, noted above, are preponderately right in structure and sexual. But the spirogyras are nearly all left in structure and reproduce sexually, although parthenogenesis is also present.

Now taking together all the observations on the relation of sex to spiral structure, involving a number of groups of animals and plants, totaling about 17,500 species, of which the most of course are gastropods, there is seen to exist a comparatively high degree of correlation between the presence of left spiral structure and (a) absence of sex, or (b) marked reduction of sexual equality, or (c) hermaphroditism, on the one hand; and right spiral structure and equality of sex on the other. In the light of the preceding experiments and observations, this points to the very interesting possibility that sex is also fundamentally a molecular property, usually associated with the right stereoisomeric molecule.

We have here then 6 sets of observations which are definitely interrelated statistically. At the bottom of these relationships lie gross spiral structure and spiral movement. Of the 6 sets of data mentioned here, the first 2 sets, on periodicity and light effects are experimental and of course the most definite in pointing to the molecule as the mechanism underlying the observed reactions. The other sets of general observations, so far as they go, support this conclusion, and they are just such correlations as one would expect to find if the general principles of stereochemistry hold also in stereo-biology.

Now an interpretation broad enough to include all these data requires that the protoplasm of amebas, and apparently of other organisms as well, be assumed to consist primarily of molecules, specific chemically for the species, which are organized into definite patterns. The molecules must consist apparently of two fundamental types: right stereoisomers and left stereoisomers, which are not merely spatially different, but like the other stereoisomers of chemistry, they must also differ in some degree in other respects. (Many other stereoisomeric conditions less deep-seated than these could of course exist.) These isomers, in the case of organisms other than amebas, may give rise, most conspicuously, to a spiral structure of the body, which in many cases is more or less masked;

second, the right isomer readily falls into the two sexual phases, while the left does so comparatively rarely.

The organizing process of molecules at the forward end of the moving ameba is, in consequence, assumed to be accompanied by the formation of fields which derive their characteristics from the nature of the molecules; for there can be no (spontaneous) molecular organization without an accompanying field (comprising sub-fields). This field is the "organized aspect" of the organism. It follows from this that not only some but most or all of the characteristics of the organism are due to, or rather *correlated with positional relationships of the molecules*, and ultimately with the stereo-relations of the atoms of the molecules. It is, in fact, theoretically unique to suppose that a mass of matter, of whatever size, in a system (such as a molecule or an organism) can give rise to a particular characteristic of that system; for the characteristics of such systems as we know about are commonly held to be due to number and positional relationships of the constituent particles.

The experimental data on the amebas, on which the hypothesis of molecular organization is specifically based, will soon be published in full.

A. A. SCHAEFFER

THE BIOLOGICAL LABORATORY,
COLD SPRING HARBOR,
LONG ISLAND, NEW YORK

CERCOSPORELLA HERPOTRICHOIDES FRON, THE CAUSE OF THE COLUMBIA BASIN FOOTROT OF WINTER WHEAT

DURING the past eleven years an important footrot of winter wheat and barley has been under investigation in eastern Washington and Oregon. The symptoms already have been described in part.[1] An undetermined fungus, producing sterile, moderately slow-growing, compact, smoke-gray, mycelial colonies on Difco potato-dextrose agar, has been consistently isolated from young lesions that occur in the basal nodes of the culm. In special studies with this fungus conducted by the writer during the fall and winter of 1929 and again in the fall of 1930, at Corvallis, Oregon, the fungus sporulated profusely when grown on cornmeal and incubated outdoors. The fungus proved to be *Cercosporella herpotrichoides* Fron. Conidia were produced in slimy, pink masses, or pseudopionnotes, at the edges of colonies four weeks old. Conidia were developed also on loosely formed coremium-like structures, on sporodochia or microsclerotia, and to some extent on independent hyphae. The spores germinated at one or both ends and, in less than a week, produced the characteristic, smoke-gray colonies on Difco potato-dextrose agar.

Early in the spring of 1930, spores of *Cercosporella herpotrichoides* were found on lesions at the bases of culms of naturally infected wheat plants in the field near The Dalles, Oregon, and also on artificially inoculated wheat plants in the greenhouse at Corvallis, Oregon.

Artificial inoculations on wheat were made, in 1929 and 1930, in the greenhouse and in the field at Corvallis, Oregon, and in the field at three different points in and near The Dalles, Oregon. The fungus was carried through five mono-mycelial transfers before starting the pathogenicity tests. As inoculum for pathogenicity tests, the fungus was grown in quantity in flasks on a sterilized mixture of oats and barley kernels. In making the inoculations the fresh

TABLE 1

RESULTS OF INOCULATING WHEAT PLANTS WITH *Cercosporella herpotrichoides* AT CORVALLIS AND THE DALLES, OREGON, 1929–1930

Location	Distance from nearest known naturally infected area, miles	When inoculated	Results obtained
Corvallis, Ore.:			
In greenhouse	120	At time of seeding, Oct., 1929, to Feb., 1930 Oct., 1930, to Jan., 1931	Typical footrot symptoms and also stunting and frequent death of seedlings
On college farm	120	Jan. 3, 1930, 1 month after seeding	Typical footrot symptoms, including falling of culms
The Dalles, Ore.:	2		
In the country, elevation 2,800 feet		Oct. 10, 1929, 6 weeks after seeding	Typical footrot symptoms except no falling
In the country, elevation 2,200 feet	5	Oct. 10, 1929, 6 weeks after seeding	Typical symptoms, including falling
In a garden in The Dalles, elevation 150 feet	10	March 15, 1930, 5 months after seeding	Footrot lesions near maturity

[1] H. H. McKinney, "Footrot Diseases of Wheat in America," U. S. Dept. Agr. *Bull.* 1347: 28–30, 1925.

inoculum was crumbled and mixed into the soil at seeding time or later. The results of these inoculations are given in Table 1. In each case, uninoculated wheat plants grown as controls remained healthy throughout and the characteristic *Cercosporella herpotrichoides* was isolated from diseased wheat plants from the inoculated series at each place.

In a paper by Fron[2] in 1912, it is held that *Cercosporella herpotrichoides* Fron is the conidial stage of *Leptosphaeria herpotrichoides* De Not. However, in a subsequent paper, Foëx,[3] unable to substantiate Fron's claim, holds that the two fungi are not genetically connected. In a recent paper by Foëx and Rosella,[4] an undetermined fungus from "eye-spot" lesions (le champignon des taches ocellées) is described that agrees, in cultural characters, with the *Cercosporella herpotrichoides* being studied by the writer. Foëx and Rosella designate their organism as "champignon X." They also found *Cercosporella* spores similar to *C. herpotrichoides* on wheat plants grown in soil artificially inoculated with "champignon X." These writers state, however, that it is still uncertain whether "champignon X," the undetermined "eye-spot" fungus, belongs to this species or not. Dr. Foëx recently sent the writer transfers of a pure culture of "champignon X" and it proves to be microscopically identical with the pure cultures of *Cercosporella herpotrichoides* isolated by the writer from diseased wheat in Oregon.

The only ascigerous stage of a suspected parasite that has been found on wheat footrots in the Columbia Basin area, aside from traces of *Ophiobolus graminis,* is a small amount of *Leptosphaeria herpotrichoides* De Not. found by McKinney[5] in Spokane County, Washington. A culture of a *Leptosphaeria* sent from Canada, referred to by Henry and Foster,[6] is distinct in appearance, on potato-dextrose agar, from the *Cercosporella herpotrichoides* studied by the writer and similarly different also from the culture received by the writer from Dr. Foëx. The *Leptosphaeria* from Canada also differs from *C. herpotrichoides* in rate of growth on agar, at room temperatures, and in pathogenicity.

Fron did not give a formal technical description of *C. herpotrichoides* when he named the fungus, and correspondence[7] has failed to locate any type material. Fron's discussion and drawings lead the writer to conclude, however, that the fungus causing the Columbia Basin footrot is taxonomically identical with *Cercosporella herpotrichoides* Fron. Because of its prevalence in literature, the writer prefers to retain Fron's name and submits the following emended description based on the fungus as it occurs in Washington and Oregon.

CERCOSPORELLA HERPOTRICHOIDES FRON. (EMENDED)

Fawn color to white, brown-bordered elongate (rarely circular) spots up to 3 cm in length, occurring on outer leaf sheaths at base of culms, later penetrating into interior of stems in spring of year or earlier, causing light-colored, later brown to black, charred-appearing lesions at the ground level, rarely higher; margins of lesions dark brown to golden brown; tissue of lesions firm and brittle, later sunken, culms eventually falling.

Mycelia septate, of two sorts: (1) Vegetative, yellow to dark brown, linear-celled; (2) stromatic, medium to very heavy walled, frequently consisting of polygon-shaped cells forming charred masses on the outside of stems or sheaths or occurring in cells in the interior of attacked culms. Conidiophores simple to slightly branched, sometimes swollen at the base and elongated, produced from macrohyphae subicula. Spores usually produced in spring of year on spots or lesions, frequently in pairs, often singly, somewhat curved, obclavate, two to several celled (mostly 5 to 7), variable, 1.5–3.5 x 30–80 μ (mostly 40–60 μ). On cornmeal (*Zea mays*), sporulation occurs in cool fall or winter weather, spores are produced in pseudopionnotes, coremia and loosely-formed sporodochial pads; spores strongly obclavate, blunt, sometimes pointed at apex, cells sharply constricted, somewhat doliform, 1.5–3.5 x 20–45 μ; on potato-dextrose agar, spores germinate usually from terminal cells but sometimes from any cell, and produce smoke-gray colonies of compact mycelia.

On culms of *Triticum vulgare* Vill. and *Hordeum vulgare* L. in Oregon, Washington and Idaho.

Material from natural infection on C. A. Johnson farm, High Prairie, Klickitat County, Washington, and material in pure culture on cornmeal from monomycelial transfers from scattered areas in the Pacific Northwest have been deposited in the mycological

[2] G. Fron, "Contribution à L'Etude de la Maladie 'Pied Noir des Cereales,' ou 'Maladie du Pietin,'" *Ann. de la Sci. Agron Francaise et Etrangere,* Année 29 (Sér. 4, Annee 1), Sem. 1: 3–29, 1912.

[3] Et. Foëx, "Note sur le Pietin du Blé," *Bul.* de la Soc. Path. Veg. de France, 6: 52–56, 1919.

[4] Et. Foëx, and Et. Rosella, "Sur les diverses formes du pietin," *Rev. Path. Veg. et Ent. Agric.,* 17: 41–51, February, 1930. [Abs. in Rev. Appl. Myc., 9: 640–641, 1930.]

[5] H. H. McKinney, *loc. cit.*

[6] A. W. Henry and W. R. Foster, "Leptosphaeria Footrot of Wheat in Alberta," *Phytopath.,* 19: 689–690, 1929.

[7] The writer is indebted to Miss E. M. Wakefield, of the Royal Botanic Gardens, Kew, England, Dr. Geo. H. Pethybridge, of the Ministry of Agriculture, England, and to Dr. Et. Foëx, Institut des Recherches Agronomiques, Versailles, France, who so kindly assisted the writer in searching for type material of *C. herpotrichoides.* The writer also is indebted to Dr. Foëx for exchange cultures of footrot causing fungi.

collections of the Bureau of Plant Industry, U. S. Department of Agriculture.

RODERICK SPRAGUE
U. S. DEPARTMENT OF AGRICULTURE,
IN COOPERATION WITH THE
WASHINGTON AND OREGON AGRICULTURAL
EXPERIMENT STATIONS

THE SEX RATIO AMONG HUMAN STILLBIRTHS

IT has long been known that among human births there is a marked and fairly constant excess of males over females, the observed sex ratio for living births in the United States, for example, being about 105 or 106 males per 100 females. This surplus of males at birth might be explained by postulating a higher intra-uterine death-rate for females than for males. An examination of the sex ratio among stillbirths,[1] however, does not support this view; on the contrary, it discloses an even greater excess of males among those gestations which are interrupted prematurely than among those which terminate in normal, living births. The published data on this subject are adequately summarized in a paper by Holmes and Goff[2] and by Schultz[3] in his comprehensive study of the sex incidence in abortions, to which the reader is referred for a more detailed discussion of the matter.

The purpose of this paper is to call attention to the sex ratio computed from official statistics[4] covering 51,810 stillbirths which occurred in a selected area[5] of the United States, during the years 1926, 1927 and 1928. In Table I these cases are arranged according to their recorded sex and age. To aid in

TABLE I

STILLBIRTHS FROM A LIMITED AREA* OF THE UNITED STATES DURING 1926, 1927 AND 1928, SHOWING THE NUMBER OF MALES AND FEMALES AND THE SEX RATIO, ARRANGED ACCORDING TO THE PERIOD OF UTERO-GESTATION

	Under 4 months			4 months		
	♂	♀	Sex ratio	♂	♀	Sex ratio
1926	170	52	326.92	359	165	217.57
1927	222	58	382.75	405	196	206.63
1928	205	57	359.64	470	192	244.79
Total	597	167	357.48	1,234	553	223.14

	5 months			6 months		
	♂	♀	Sex ratio	♂	♀	Sex ratio
1926	678	473	143.34	881	653	134.91
1927	756	541	139.74	1,096	868	126.26
1928	839	617	135.98	1,117	879	127.07
Total	2,273	1,631	139.36	3,094	2,400	128.91

	7 months			8 months		
	♂	♀	Sex ratio	♂	♀	Sex ratio
1926	1,052	941	111.79	1,184	989	119.71
1927	1,364	1,144	119.23	1,486	1,113	133.51
1928	1,340	1,136	117.95	1,421	1,164	122.07
Total	3,756	3,221	116.60	4,091	3,266	125.26

	9 months			10 months		
	♂	♀	Sex ratio	♂	♀	Sex ratio
1926	4,278	3,107	137.68	205	126	162.69
1927	5,088	3,712	137.06	186	126	147.61
1928	4,864	3,535	137.59	175	125	140.00
Total	14,230	10,354	137.43	566	377	150.13

SUMMARY

Age	♂	♀	Sex ratio
Less than 4 months	597	167	357.48
4 months	1,234	553	223.14
5 months	2,273	1,631	139.36
6 months	3,094	2,400	128.91
7 months	3,756	3,221	116.60
8 months	4,091	3,266	125.26
9 months	14,230	10,354	137.43
10 months or more	566	377	150.13
Total	29,841	21,969	135.83

* Including Connecticut, Illinois, New Jersey, Oregon, Utah, Washington, Baltimore, Maryland, District of Columbia, and New York. (Figures for 1927 and 1928 include the entire state of New York; those for 1926 are for New York City only.)

[1] As employed by U. S. Bureau of the Census, the term "stillbirth" apparently includes all interruptions of pregnancy, regardless of their cause or the period at which they occurred.

[2] S. J. Holmes and J. C. Goff, "The Selective Elimination of Male Infants under Different Environmental Influences," "Eugenics in Race and State," II, 247–248, Baltimore, Williams and Wilkins Company, 1923.

[3] Adolph H. Schultz, "Sex Incidence in Abortions," Carnegie Institution Publication No. 275, Washington, 1921.

[4] Birth, Stillbirth and Infant Mortality Statistics for the Birth Registration Area of the United States, 1926, 1927 and 1928.

[5] Including Connecticut, Illinois, New Jersey, Oregon, Utah, Washington, Baltimore, Maryland, District of Columbia and New York. (Figures for 1927 and 1928 include the entire state of New York; those for 1926 are for New York City only.)

comparison, the number of individuals of each sex and the sex ratio are listed separately for each year and for each age group, while the last horizontal line of the table contains corresponding values for the three years considered together.

The average number of males per 100 females for each age group is as follows: less than 4 months, 357.48; 4 months, 223.14; 5 months, 139.36; 6 months, 128.91; 7 months, 116.60; 8 months, 125.26; 9 months, 137.43; 10 months or more, 150.13. It will be observed that in these data the excess of males among stillborn cases decreases, at first abruptly and later more gradually, up to and including the seventh month of pregnancy. At eight and at nine months, the percentage increases again, due, probably, to the fact that the somewhat larger average size of male infants increases the likelihood of their incurring fatal injury during parturition.

The very great excess of males recorded among embryos of less than four months' development should not, however, be accepted without some qualification. It is probable that many listed as males in this group are really females, and that the observer mistook the clitoris for a penis, due to the similarity in appearance of these two structures during early development. Since, however, it is possible to distinguish between the sexes of human embryos as young as six or seven weeks, on the basis of the relative length of the urethral groove, the angle at which the phallus meets the body, etc., the sex of the majority of cases at which a qualified physician was in attendance was probably correctly diagnosed. But we do not know what proportion of these earliest cases were examined by competent observers, so it can not be determined to what extent the apparent sex ratio of the group has been affected by this source of error. Since, however, the sex ratio during the fourth, fifth, sixth and seventh months shows a constantly decreasing excess of stillborn males, it seems reasonable to assume that the true sex ratio among those of less than four months' development is definitely higher than that of the next older group, in which the sexes should not easily be confused. It is likely that the recorded age of embryos and fetuses in this material is based on estimates by the mother rather than on accurate measurements, yet in so large a number of cases this procedure should introduce no serious error.

The findings of this study agree essentially with those of Nichols[6] and of Schultz,[3] in showing that the excess of males among stillborn children is much lower during the middle third of intra-uterine development than during either the first or the last third of it. They suggest, too, that the wastage of male embryos during the first three months of pregnancy must be very great.

Much interest attaches to the sex ratio among stillbirths, especially among those which occur during the early months of gestation. The available evidence concerning the relative number of stillborn males and females during the later months of pregnancy proves conclusively that the excess of the former sex at the time of conception is even greater than is indicated by the sex ratio among living births. Information concerning the relative mortality of the two sexes during the first three months of development is of even greater importance in this connection, for more than 50 per cent. of all abortions are supposed to occur during this time,[3] and it has been estimated that there is one abortion for every 4.5 pregnancies which proceed to term.[7] Unfortunately, this is the very period for which the available statistics are least reliable. The majority of early abortions are probably concealed; and, of those which are reported, information as to sex must, of course, be limited to those cases which occur after the external genitalia are distinguishable, *i.e.*, after the sixth week of development. As pointed out above, however, there is reason to question the accuracy of the reported sex of even those cases.

Once we have some reliable data on the sex of stillborn embryos during the period from the sixth to the twelfth weeks of intra-uterine development, it may be possible to estimate fairly accurately the sex ratio at the time of conception. Such an estimate must, however, await the compilation of a large number of records of early cases in which the age and sex are known to have been determined by competent observers.

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UNIVERSITY OF COLORADO

[6] John B. Nichols, "The Numerical Proportions of the Sexes at Birth," Memoirs of the American Anthropological Assn., Vol. I, 267.

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